



**THE
FOURTH
LACUS
FORUM
1977**



**THE FOURTH
LACUS FORUM
1977**

Edenda curant

ADAM MAKKAI

Executive Director and Director of Publications

&

VALERIE BECKER MAKKAI

Secretary-Treasurer and Co-Ordinating Editor

THE FOURTH LACUS FORUM 1977

edited by Michel Paradis



HORNBEAM PRESS, INCORPORATED
6520 COURTWOOD DRIVE • COLUMBIA, SOUTH CAROLINA 29206

Copyright © 1978 by the Linguistic Association of Canada and the United States.

FIRST EDITION

Published in Columbia, South Carolina by Hornbeam Press, Incorporated,
6520 Courtwood Drive, Columbia, S. C. 29206.

Manufactured in the United States of America.

CONTENTS

Introduction: The Interdisciplinarity of Linguistic Studies	ix
1977 Presidential Address	1
<i>Roger Williams Wescott</i>	
THE LINK OF LANGUAGE	3
I. Developmental Psycholinguistics: The Acquisition of Language	17
1. <i>Vit Bubenik</i>	
THE ACQUISITION OF CZECH IN THE ENGLISH ENVIRONMENT	19
2. <i>James Cummins</i>	
METALINGUISTIC DEVELOPMENT OF CHILDREN IN BILINGUAL EDUCATION PROGRAMS: DATA FROM IRISH AND CANADIAN UKRAINIAN-ENGLISH PROGRAMS	29
3. <i>Patsy M. Lightbown</i>	
QUESTION FORM AND QUESTION FUNCTION IN THE SPEECH OF YOUNG FRENCH L2 LEARNERS	41
4. <i>Donald Preziosi</i>	
LANGUAGE AND PERCEPTION	51
5. <i>Valerie Becker Makkai</i>	
MATERNAL DIGLOSSIA: THE ADULT USE OF "BABY TALK" AS AN INSTRUCTIONAL DEVICE	61
II. Psycho- and Neurolinguistics: Language and the Brain	67
6. <i>Gary S. Dell</i>	
SLIPS OF THE MIND	69
7. <i>Joseph F. Kess and Ronald A. Hoppe</i>	
PSYCHOLINGUISTIC EXPERIMENTS IN AMBIGUITY: ANALOGY, ANOMALY, OR ARTIFACT?	75
8. <i>Bruce Bain and Agnes Yu</i>	
TOWARD AN INTEGRATION OF PIAGET AND VYGOTSKY: A CROSS-CULTURAL REPLICATION (FRANCE, GERMANY, CANADA) CONCERNING COGNITIVE CONSEQUENCES OF BILINGUALITY	83
9. <i>John L. Debes</i>	
SOME COMMON TRAITS OF VISUAL AND VERBAL LANGUAGE AND LANGUAGING	97
10. <i>Lorraine K. Obler and Martin L. Albert</i>	
A MONITOR SYSTEM FOR BILINGUAL LANGUAGE PROCESSING	105
11. <i>Marc L. Schnitzer</i>	
CEREBRAL LATERALIZATION AND PLASTICITY: THEIR RELEVANCE TO LANGUAGE ACQUISITION	114
12. <i>Linda Galloway</i>	
LANGUAGE IMPAIRMENT AND RECOVERY IN POLY-GLOT APHASIA: A CASE STUDY OF A HEPTA-LINGUAL	121

III.	Linguistics Applied to Language Teaching and Planning	131
13.	<i>Robert J. Di Pietro</i> BILINGUAL EDUCATION: A NEW CHALLENGE TO OLD PRIORITIES	133
14.	<i>Ronald C. Morren and Leon E. Williamson</i> PROCEDURES INVOLVED IN DEVELOPING LITERACY MATERIALS FOR A PRELITERATE SOCIETY	142
15.	<i>K. Connors, N. Ménard, and R. Singh, with the collaboration of D. Beaugregard, R. Lazure, and J. Morse-Chevrier</i> TESTING LINGUISTIC AND FUNCTIONAL COMPETENCE IN IMMERSION PROGRAMS	154
16.	<i>Joan Neuburger Savitt</i> POUR UNE PRISE DE CONSCIENCE DES HABITUDES ARTICULATOIRES MATERNELLES DANS L'ETUDE D'UNE LANGUE SECONDE	165
IV.	Sociolinguistics: Social Variables Influencing Language Use	173
17.	<i>Beth Bethel and Bettie Horne</i> THE LANGUAGE OF MALE-FEMALE HUMOR IN POPULAR PULP	175
18.	<i>Lili Rabel-Heymann</i> BUT HOW DOES A BILINGUAL FEEL? REFLECTIONS ON LINGUISTIC ATTITUDES OF IMMIGRANT ACADEMICS	185
19.	<i>Bernard Saint-Jacques</i> ELICITATION OF CULTURAL STEREOTYPES THROUGH THE PRESENTATION OF VOICES IN TWO LANGUAGES	194
20.	<i>Walburga von Raffler-Engel</i> CHILDREN HAVE BODIES	200
21.	<i>Madeleine Mathiot and Eleanor Dougherty</i> A FUNCTIONAL EMPIRICIST APPROACH TO THE ANALYSIS OF FACE-TO-FACE INTERACTION	209
22.	<i>David L. Lawton</i> BILINGUAL STRATEGIES OF COMMUNICATION: EVIDENCE FROM THE TEXT	218
23.	<i>Saul Levin</i> SCHOLARLY AND LITERARY BILINGUALISM	226
24.	<i>Lenora A. Timm</i> CODE-SWITCHING IN <i>WAR AND PEACE</i>	236
25.	<i>The Second Foundation</i> RELATIONAL NETWORK APPROACHES TO CODE- SWITCHING	250
26.	<i>John M. Lipski</i> CODE SWITCHING AND THE PROBLEM OF BILINGUAL COMPETENCE	263
27.	<i>Nicole Domingue</i> L'USAGE BILINGUE DANS LE CENTRE DE MONTREAL	278
28.	<i>Jon Amastae and Lucía Elías-Olivares</i> ATTITUDES TOWARD VARIETIES OF SPANISH	286
29.	<i>Reza Ordoubadian</i> AZARBAYJANI: BILINGUALISM AND POLITICS	303
30.	<i>Carol P. Hines</i> WELL, . . .	308

31.	<i>Wolfgang Wölck</i>	
	TOWARDS A CLASSIFICATORY MATRIX FOR LINGUISTIC VARIETIES, OR: THE DIALECT/LANGUAGE PROBLEM REVISITED	319
V.	Linguistic Metatheory: Epistemological Foundations of Linguistics	329
32.	<i>Paul L. Garvin</i>	
	AN EMPIRICIST EPISTEMOLOGY FOR LINGUISTICS	331
33.	<i>Hans R. Bergan</i>	
	TRANSMUTATION AS A DEFINING CHARACTERISTIC OF LANGUAGE	352
34.	<i>Joe Darwin Palmer</i>	
	LINGUISTICS IN MEDIAS RES	363
35.	<i>Charles Ruhl</i>	
	TWO FORMS OF REDUCTIONISM	370
36.	<i>Bruce L. Pearson</i>	
	PARADIGMS AND REVOLUTIONS IN LINGUISTICS	384
37.	<i>Alexander T. Borgida</i>	
	FORMAL ASPECTS OF STRATIFICATIONAL THEORY	391
VI.	Diachronic Linguistics: Historical and Comparative Linguistics	401
38.	<i>Stephen Tyma</i>	
	RELATING SYNCHRONY AND DIACHRONY IN THE ANALYSIS AND DESCRIPTION OF LANGUAGE	403
39.	<i>Carleton T. Hodge</i>	
	LISLAKH	414
40.	<i>Connie C. Eble</i>	
	AELFRIC AND BILINGUALISM IN ANGLO-SAXON ENGLAND	423
VII.	Grammar: Phonology, Morphology, Syntax and Semantics	433
41.	<i>Jean E. Newman</i>	
	THE USE OF CONTRASTIVE STRESS IN DIALOGUE: AN EXPERIMENT	435
42.	<i>Sandra C. Browne</i>	
	PHONOLOGICAL ASPECTS OF TIMING	444
43.	<i>William J. Sullivan</i>	
	THE "ENCLITIC" SYLLABLE IN ENGLISH	451
44.	<i>Joseph Oliva and Judith Duchan</i>	
	THREE LEVELS OF TEMPORAL STRUCTURING IN SPOKEN LANGUAGE	460
45.	<i>Mervin Barnes and Helmut Esau</i>	
	GENERATIVE APPROACHES TO ENGLISH PROSODY: ASSESSMENT AND OUTLOOK	469
46.	<i>T. D. Griffen</i>	
	THE CASE AGAINST ALLOPHONY	484
47.	<i>M. Carmen Lozano</i>	
	AN ARGUMENT FOR PARTIAL SPECIFICATIONS: EVIDENCE FROM SPANISH	491
48.	<i>Melvin J. Hoffman</i>	
	A DIFFERENT PERSPECTIVE TOWARD: "ECONOMY AND SYMMETRY IN ENGLISH CONSONANTS"	502

49.	<i>Yakov Malkiel</i>	
	FROM PHONOSYMBOLISM TO MORPHOSYMBOLISM	511
50.	<i>Laurent Santerre, Danielle Noiseux and Luc Ostiguy</i>	
	LA CHUTE DU /1/ DANS LES ARTICLES ET LES PRONOMS CLITIQUES EN FRANCAIS QUEBECOIS	530
51.	<i>Jean-Luc Garneau</i>	
	FRENCH AND ENGLISH PREPOSITIONS: A BI-STRATAL STUDY	539
52.	<i>Raymond Mougéon, Monique Bélanger, and Michael Canale</i>	
	LE RÔLE DE L'INTERFÉRENCE DANS L'EMPLOI DES PREPOSITIONS EN FRANCAIS ET EN ANGLAIS PAR DES JEUNES FRANCO-ONTARIENS BILINGUES	550
53.	<i>Hans-Heinrich Lieb</i>	
	ON THE SYNTAX AND SEMANTICS OF GERMAN MODAL VERBS: A SURFACE STRUCTURE ANALYSIS	560
54.	<i>Christine M. Andrew</i>	
	AN EXPERIMENT ON FOCUS	576
55.	<i>Ulf Bäcklund</i>	
	THE RELATION BETWEEN LINGUISTIC PROXIMITY AND EXTRALINGUISTIC CONTRAST	585
56.	<i>Betty Lou Dubois</i>	
	A CLOZE STUDY OF VERB USAGE IN CONTEXT: ANGLOS AND CHICANOS	604
57.	<i>Allen Walker Read</i>	
	THE NUMERICAL NAMING OF PEOPLE	615
VIII.	Text Linguistics: Discourse Analysis Beyond the Sentence	625
58.	<i>Brian Phillips</i>	
	A CALCULUS OF COHESION	627
59.	<i>Terence H. Wilbur</i>	
	THE GRAMMAR OF THE BASQUE PROVERB	638
60.	<i>Deborah Tyma</i>	
	BEYOND THE FRAGMENT: USING STRATIFICATIONAL AND TAGMEMIC THEORY IN AN ANALYSIS OF PASCAL'S <i>PENSEES</i>	642
61.	<i>Doris Lander</i>	
	GRAMMATICAL VERSUS REFERENTIAL STRUCTURE ILLUSTRATED IN ENGLISH DEFINITION	650
62.	<i>Jolande Kelemen</i>	
	QUELQUES ASPECTS D'UNE STYLISTIQUE COMPAREE DU FRANCAIS ET DU HONGROIS A LA BASE DE LA LINGUISTIQUE	660
63.	<i>Adam Makkai</i>	
	THE MEANING OF NOEL COWARD'S "VENICE"	670

INTRODUCTION

The Fourth LACUS Forum, held at McGill University in Montreal from the 13th to the 17th of August, was attended by over 140 participants. This high attendance reflects the 60% increase in membership recorded over the past twelve months.

One of the original intentions of the founders of the Linguistic Association of Canada and the United States was to broaden the scope of linguistic studies. That they are well on their way to achieving their goal may be inferred from the growing appeal that the association has had in its four years of existence, and from the variety of topics that were covered in the papers presented at this year's conference, which are published in this volume.

Roger Wescott, in his presidential address, viewed linguistics as a link between disciplines. While listening to him, it became clear to me that one of the main characteristics of our association is a particular awareness on the part of its members of the ineluctability of the interdisciplinary aspect of linguistic studies. The structure of language, from sound patterns to the organization of meanings, is studied with a view to placing it in the wider context of human communication, where it is necessarily affected by influences outside of linguistic structure proper. Every member, irrespective of theoretical background, attempts to relate his or her linguistic research to the biological, psychological, aesthetic, philosophical, social or political study of man. This is why so many of us are working in hyphenated linguistics (and the hyphen by now, has most often been dropped): either in neuro-linguistics, psycholinguistics, sociolinguistics, anthropological linguistics, computational linguistics, mathematical linguistics, or in any area where linguistics is seen in relation to other disciplines, such as epistemology, logic, literature or pedagogy.

Paul Garvin has remarked elsewhere that the major contribution of neuro-linguistics to linguistics is "a link to the physiological substratum of language and a *means of validation*" (my italics). Harry Whitaker, likewise, considers that "it is not acceptable to remove linguistic hypotheses to an abstract level outside of neurological correlation and refutation." The same may be said of sociolinguistics and some conference participants have argued that there is no linguistic competence that does not incorporate sociolinguistic variables.

The interdisciplinarity of the majority of the papers presented at the Fourth LACUS Forum attest to the fact that LACUSians are very much aware of the "link" of language. These papers have been grouped under eight headings, which, like language itself, are neither totally arbitrary nor logically necessary, but contingent upon a number of factors. Some, for instance, could have been classified under two or more headings. They may find themselves in one category for no better reason than the aesthetic satisfaction of having sections of not too divergent sizes, and in an effort to keep their number to a minimum.

The Inaugural Lecture was delivered in French by Professor Paul L. Garvin. Professor Yakov Malkiel gave the Invited Lecture.

Twenty-five of the 64 papers that were actually read at the Forum dealt directly with some aspect of bilingualism. These papers, together with papers from the *Second* and the *Third LACUS Forum*, as well as some original contributions, are simultaneously published by Hornbeam Press in a separate volume, under the title, *Aspects of Bilingualism*, the theme of this year's conference.

It was most appropriate that Montreal should have been chosen as the place

for discussion of such a theme, at a time when much of the Province's concern focuses on language issues, in a city where not only the French and the English meet, but the Greek, the Italian, the Chinese, and representatives from just about every language group and culture of the world, as tangibly illustrated by the variety of restaurants in the immediate vicinity of the campus that were sampled by conference participants.

The emblem of the city (reproduced on the cover) pays tribute to the multiplicity of ethnic origins of its founders, displaying a French fleur de lys, an English wild rose, a Scottish thistle and an Irish shamrock.

On behalf of all participants, I wish to thank Dr. Robert Vogel, Dean of the Faculty of Arts, Dr. Walter Hitschfeld, Dean of the Faculty of Graduate Studies and Research, and Dr. C. Douglas Ellis, Chairman of the Department of Linguistics for their moral and financial support, and Dr. Donald Little, Assistant Director, Institute of Islamic Studies, for allowing us to use the departmental lounge and adjacent seminar room for the book display. I also wish to express our gratitude to the Canada Council for their grant Nr. 443-770048 toward the travel expenses of members residing in Canada.

Michel Paradis
September 1, 1977

REFERENCES

- Garvin, Paul L., "Summation and Review", in E. C. Carterette (ed.), *Brain Function*, vol. III: Language, Speech and Communication, University of California Press, 1966, p. 263.
- Whitaker, Harry A., "A Model for Neurolinguistics", *Occasional Papers* (Language Centre, University of Essex, Colchester), 10 (1970), p. 9.

1977 PRESIDENTIAL ADDRESS

THE LINK OF LANGUAGE

Roger Williams Wescott
Drew University

Before explaining the title of my talk today, I should like to remark briefly on a wide-spread divergence among the membership of our society with regard to the preferred pronunciation of the acronym LACUS. Some of us pronounce it so as to rhyme with "shake us"; others, with "whack us"; and still others, with "mock us". Trivial as the matter may seem, it must be admitted that this three-way disparity has produced at least a minor *frācas* -- or *frācas* -- alias *frācas*. Pondering this unforeseen disarray in our ranks, I found it (prone as I am, in Alexander Pope's words, to "lisp in numbers") hard to resist phrasing the problem in verse. The result was

Lacusian confusion

or

a verse disquisition on a phonemic-graphemic disjunction

If we claim to hail from /léykǽs/,
Variant vocalisms rake us.
If, instead, we speak of /lækǽs/,
Other renderings may wrack us.
Yet, if we are fans of /lākǽs/,
Both the former two will rock us.
All three, though, agree on */lǒhkǽs/:
To pronounce it so is raucous!

To return, however, to my title: What first suggested "the link of language" to me was the surprisingly large number of misprints I encountered of the word linguistics as "linquistics." Not only have I received numerous typed letters addressed to me in my capacity as a teacher of "linquistics", but I have repeatedly observed the word thus misspelled in even such prestigious (though predictably non-linguistic) scholarly journals as Science. A recurrent typo of this sort is probably best explained in terms of the far greater frequency, in written English, of the sequence <qui>, as in quilt, than of <gui>, as in guilt.

However that may be, I am grateful to my heterographic benefactors for providing me with "the link of language," because I not only see language as a link between people but also perceive linguistics -- at its best, in any case -- as a link between disciplines.

My fellow Lacusians, especially those who heard him speak in Juarez, Mexico, last August, will of course note the titular similarity between this talk and Dwight Bolinger's delightful address "The Price of Language." Both he and I, to be sure, are following in venerable footsteps when we speak of "the X of language," where X is a noun or noun-phrase. To mention only the few that I reread in preparation for this evening, I may cite William Dwight Whitney's Life and Growth of Language (1875), Henry Sweet's History of Language (1900), Frederick Bodmer's Loom of Language (1944), and John Hughes' Science of Language (1962). Of the four, I found Sweet most congenial. In fact, I found him advancing, quite unapologetically, six of the theoretical positions which I have often found myself beleaguered in defending. These are, in the order in which Sweet presented them: (1) that language consists of a core, corresponding to George Trager's "microlanguage,"¹ and a periphery, corresponding to my "allo-language";² (2) that manual sign-language is just as linguistic as speech is; (3) that language is iconic, or imitative, as well as symbolic, or arbitrary; (4) that Indo-European (which he, like me, preferred to call Aryan, to obviate the inference of an Indic vs. European bifurcation) had had a pre-inflectional stage; (5) that Semitic and Egyptian trilateral stems are derived from biliteral roots with uniliteral suffixes; and (6) that Indo-European and Uralic share too many basic lexical elements to permit reductive explanation in terms of either borrowing or coincidence.

Before leaving the topic of the price of language, I should like to register my agreement with Dwight Bolinger's thesis that language, like all other utilities, does exact a price. In addition to the linguistic costs which he mentioned, I would like to add at least one more: the fact that language, as a predominantly left-brained activity, tends to inhibit right-brained communicative skills of the type generally called non-verbal. Psychotherapists are especially aware of this problem with regard to those patients who are articulate to the point of being "hyper-verbal" and consequently adept at using words as blinds to shut out liberating insights. Moreover, if telepathy, or direct mind-to-mind communication between individuals, is a real human faculty, as an increasing number of investigators appear to think,³ it seems very likely that its "still, small voice" is drowned out by mouth-to-ear, if not by pen-to-eye, communication.

Returning to language as a link, I think it safe to presume that an overwhelming majority of LACUS members are not specialists in language alone but in language and -- language and theology, language and literature, language and culture, language and behavior, or language and the brain, to mention only a few of the intellectual couplings in which we are variously involved. Many of us, especially those who have recent doctorates from prestigious linguistics departments, may regard our current assignments, teaching courses in English, foreign languages, or social sciences,

as regrettable but only temporary dilutions of our scholarly purity, from which we must eventually be rescued by a call to exclusively linguistic research. If so, ours is a predicament to which that old and often overworked phrase "a blessing in disguise" may, I think, quite appropriately be applied. In commenting on the Cartesian tradition in Western thought, Norman O. Brown once remarked that "pure reason is, in principle, pure madness." In the same spirit, I maintain that pure linguistics is, in principle, pure pedantry. To achieve not only practical relevance but intellectual vitality, linguistics must, I believe, be firmly linked to subject matters and disciplines other than those of language.

In a sense, of course, linguistics already is so linked. Since the time of Panini, linguistics has, through its exegesis of sacred texts, been involved in religious studies. Since the time of Aristophanes of Byzantium, it has, through the preparation of critical editions, been involved in literary studies. And, since the 19th century, its involvement with prehistoric studies, speech physiology, and acoustic phonetics has tied linguistics in various ways to the social, biological, and physical sciences. In principle, therefore, linguistics has, for at least a century, spanned the entire spectrum of academic disciplines from theology to physics. This range has made it, in theory, an almost perfect enterprise for polymaths. In practice, however, most linguists have all too readily succumbed to the fierce pressure to specialize which afflicts not only academic but nearly every other technical enterprise in our civilization. The result has been a situation in which phoneticians cannot converse with grammarians nor psycholinguists with philologists. It is my hope that the countervailing pressure toward practical application may at least partially offset this trend toward fragmentation.

Defining Language

All discussions of the relation between linguistic and non-linguistic reality, however, are heavily handicapped by the lack of a contemporary consensus regarding the definition of language. Laymen in search of an authoritative definition doubtless think it strange that professional linguists are of so little help to them in the course of that search. In extenuation of my profession, I can only suggest that, till recently at least, the difference between human verbalisms and the signalings of other species seemed both so vast and so obvious as to require no laboring by experts -- especially when there were so many other urgent problems, both theoretical and practical, to be solved.

When pressed, to be sure, most American linguists, as represented in the glossaries of Eric Hamp⁴ and Mario Pei,⁵ have concurred in a rough definition of language as a stratified system of human communication by means of vocal symbols. Like many structuralists and most stratificationalists, I have no difficulty

with language as a stratified system, since I know of no language that lacks the phoneme/morpheme dichotomy or its equivalent (such as a cheireme /sign dichotomy).⁶ The equation of language with human communication is more problematic, in view of the fact that young chimpanzees⁷ and gorillas⁸ are now learning American Sign Language at a juvenile level and doing so, moreover, in a creative manner that excludes explanation in terms of simple conditioning.⁹ Finally, the restriction of language to vocal symbolism is, to me, quite unacceptable, because I regard both cheiresis (or manual signing) and writing as fully linguistic and also because I find significant iconic, or non-arbitrary, components in language in addition to obvious symbolic, or arbitrary, components.¹⁰

Yet, even if we agree that language is not exclusively vocal, symbolic, or human, it remains a question what, in positive terms, it is. For it is a truism that definitions are easier to criticize than to construct. Just as grammars leak, definitions creak. Nonetheless, the very effort to define can itself be productive, if only in the sense that it makes the definer more aware than he might otherwise be of the empirical and conceptual problems involved. In the late 1960's, I made such an effort to enlighten myself about the elusive nature of language by defining it in terms of twenty distinctive traits, as below:

DISTINCTIVE TRAITS OF LANGUAGE
(Speech, Signing, and Writing)

<u>Trait</u>	<u>Explication</u>	<u>Exponent</u>
I. complexity		
1. grammaticality	fixed sequence	Jespersen
2. stratification	'phoneme' vs. morpheme	Hockett
3. dichotomy	subject vs. predicate	Buehler
4. deuterism	talking about talking	Bateson
5. multicanalicity	speech vs. writing	Sebeok
6. multimodality	statement vs. command	Greenberg
7. productivity	ease of innovation	Russell
8. pantopicality	unlimited reference	Greenberg
9. lengthlessness	unlimited discourse	Greenberg
10. propositionality	equative assertion	Piaget
II. flexibility		
11. displacement	reference to the imperceptible	Hockett
12. prevarication	verbal deception	Humboldt
13. soliloquism	talking to oneself	Bronowski
14. cenemicity	absence of denotation	Hjelmslev

III. precision

15. synonymy	rephraseability	Wescott
16. referentiality	specificity of denotation	Lancaster
17. digitality	'phonemic' discreteness	Gleason
18. negation	verbal denial	Burke
19. interrogation	verbal questioning	Bronowski
20. temporality	aspect or tense	Hewes

Even a cursory examination of these traits reveals that they are distinctive, at best, only in a relative and not an absolute sense. Productivity, for example, is manifest in two non-linguistic systems of communication -- the non-human vocalization of the European song-thrush, *Turdus philomela*, and the metalinguistic art of the jazz musician, both of whom engage in melodic improvisation. It is possible, of course, to cite traits which are more distinctive in the sense that they are less likely to have non-verbal analogs; but such traits are also more likely to be trivial, in the sense that language would probably not be seriously impoverished by their absence. One such trait, proposed by Lionel Bender,¹¹ is embeddedness, or interpolation of subordinate clauses within sentences. (The notorious rarity of multiple embeddings is, I think, evidence of the dispensability of embedded constructions.)

Defining Culture

The difficulty of defining language is matched, if not exceeded, by the related difficulty of defining culture. Since culture is, if anything, an even more diffuse phenomenon than language itself, opinions on its nature have varied still more widely. Some theorists, including many British social anthropologists since the time of Alfred Radcliffe-Brown, have found culture to be as superfluous a concept as that of mind in behavioristic psychology. Most others, while agreeing on both the reality and the cruciality of culture, have been unable to agree on its substance. Two American cultural anthropologists went so far as to write a book devoted exclusively to the definition of culture -- without, however, reaching any firm conclusion on the subject.¹² In the late 19th century, definitions tended to equate culture with learning. This equation had to be given up when ethologists made it clear that virtually all animals above the level of jelly-fish can and do learn. So, in the early 20th century, culture was redefined so as to equate it with tradition, or social heredity. But this definition too was undermined by animal behavior study, which, by the 1950's, had demonstrated that most higher birds and mammals have social as well as genetic heritages. Conspicuous examples are the call inventories of crows, which change from flock to flock, and the dietary patterns of macaque monkeys, which differ from troop to troop, wholly in the absence of detectable species or subspecies differences.

Chastened by these ethological discoveries, anthropologists are now retreating into more restricted definitions of culture, equating it with verbalized tradition, inculcated tradition, and/or cumulative tradition. As a linguist, I am predictably receptive to the definition of culture as verbally transmitted social inheritance. And I find it easy to correlate verbalization with both inculcation and accumulation. For, in its more explicit forms, such as indoctrination and inducement of insight, teaching is predominantly verbal; and, at least at the lexical level, language seems to be intrinsically cumulative, ontogenetically¹³ as well as phylogenetically.¹⁴

A somewhat divergent definition of culture, with which I have been wrestling recently, equates it with mutual hypnosis. This definition is based on the highly selective nature of culture, which focuses sharp attention on some concerns while reducing or eliminating perception of others. Because it is possible to render animals cataleptic by either immobilizing them or depriving them of visual input, it may seem that this definition is incompatible (or at least incommensurable) with the preceding verbally-oriented one. But, since hypnosis in the narrow sense invariably involves some verbal cues, I think the two definitions complementary rather than contradictory. Combining them, we might reformulate our definition of culture so as to equate it with verbally inculcated and cumulatively hypnotic tradition. And even the seeming disparity between animal catalepsy and human hypnosis dwindles when we reflect how much of our social intercourse consists of a sort of verbal stroking and how much of what ethnographers call cultural blindness is a product of the verbal screens we are continually erecting between ourselves and extra-linguistic reality.

Innateness and Congenitality

Operationally, of course, an exact definition of language or culture is often unnecessary. When two investigators recently studied the manual communication systems created by untrained deaf children, they decided to avoid quibbles about whether these systems were linguistic by entitling the article embodying their findings "The Development of Language-like Communication without a Language Model."¹⁵ What they found was that all six of the two- to four-year-olds with whom they worked generated embryonic equivalents of American Sign Language. And they concluded that, unlike chimpanzees, who have to be prodded into linguistic creativity, human children exhibit "a natural inclination to develop . . . language-like communication."¹⁶

Although, ironically, he would probably reject any such source of support, these findings tend to corroborate Noam Chomsky's contention that a language-acquisition drive is innate to our species. My only caveat at this point is a suggestion that we take care to distinguish between behaviors which are

innate because they have been genetically inherited and those which are innate by virtue of having been acquired through intra-uterine learning. Hearing children, for example, usually develop adult-like speech prosody long before they acquire the phonemes of their "native" language; and it is quite possible that at least part of this prosody was acquired through prenatal auditory exposure. In the case of deaf children, however, prenatal language-learning seems virtually impossible. To avoid ambiguity, it is probably best to refer to the linguistic creativity of linguistically uninstructed deaf children as congenital rather than as innate.

Matter, Mold, and Meaning

Here, as with so many other matters of scholarly concern, the more important the question that is being posed, the less clear the evidence is and the more tentative, consequently, our conclusions must be. This, probably, is the reason why so few linguists specialize in semantics: while nothing is more important than meaning, nothing is more elusive. No other area of linguistic concern, moreover, so quickly involves its investigators in other areas of interest, such as philosophy, psychology, and folklore. And linguists, like other specialists, are queasy about stepping out of their fields. They feel wary not only of appearing contaminated in the eyes of their fellow linguists but also of being vulnerable to charges of incompetence leveled at them by experts in other disciplines.

The Linguistic Border Patrol

In our increasingly technical world, intellectual territoriality of this kind is wholly understandable. Nonetheless, I would argue that it is ultimately counterproductive. For the kinds of scholarly anomalies that lead to reformulations of scientific paradigms are most often encountered not in the solid cores of individual disciplines but in the fluid boundaries between them. At my own university, one of my favorite colleagues was a psychologist¹⁷ who used to refer to himself as a member of "the intellectual border patrol." If used by one of our more specialized co-workers, this phrase might have indicated a need to defend his discipline against encroachment by neighboring disciplines. In his case, however, it expressed an insatiable desire to widen his academic horizons. Among other courses, he taught physical anthropology. In this subdiscipline, one of the most controversial but fruitful concepts is that of hybrid vigor, a hypothesis designed to explain the apparently enhanced biological viability of mixed races. While I cannot vouch for the validity of hybrid vigor as a zoological principle, I can and do endorse it as a principle of scholarship. But I think it important to add the proviso that interdisciplinary work, to be fully fruitful, must be more than a mechanically additive juxtaposition of disciplines; it must be a dynamic and synergistic cross-fertilization of

subject-matters, of such a kind as to transform both disciplines and, eventually at least, to create new subject-matters not perceptible, even germinally, in those disciplines in their pre-fertilized state.

In these terms, I would be happy to call myself a member of "the linguistic border patrol." For the tectonic boundaries both within and around our discipline are shifting with such rapidity as to require frequent monitoring. One area that I have been exploring is what I call "allolanguage," which consists roughly of Trager's prelanguage, paralanguage, and metalanguage in combination -- or, to put it another way, of Trager's macrolanguage without his microlanguage.¹⁸ (Such exploration is not without its hazards. When I recently derived English pinky, "little finger," from Proto-Indo-European *penk^w -, "five", cognate with English fist, a colleague wrote inquiring acerbically if I had forgotten Grimm's Law. My answer, of course, is that one of the diagnostic features of allolanguage is resistance to "normal" sound-shifting. In these terms, English pa, vis-à-vis Latin pater, belongs to allolanguage, while English father belongs to microlanguage.) Another diagnostic feature of allolanguage is violation of the phonotactic constraints of microlanguage. One of the fascinations of allolinguistic dystaxis is that, in addition to the intrinsically anomalous nature of allolanguage itself, there are often anomalous fluctuations in the boundary between allolanguage and microlanguage. Thus, in Boston, yeah as an allolog of yes is fully microlinguistic, since it rhymes with there and other words in common use; whereas, in Philadelphia, yeah is allolinguistic, since it rhymes with nothing and has only the most dubious analogs in such deviant utterances as the disyllabic pronunciation of idea.

Visual Language

Another linguistic borderland, which I, like most of my fellow linguists, have explored far less, I think, than its intrinsic significance warrants, is visual language. Many structuralists, I realize, argue that systems of writing and of manual signing do not constitute language but merely represent it. But I demur. In the case of signing, most of the sceptics have simply not studied American Sign Language or any of the other richly ramified manual languages. Writing is harder to generalize about because of the fundamental disparity between iconographic and phonographic systems. In the former case, as represented by Chinese ideography, dependence on speech is minimal. Yet even in the latter, as represented by the Latin form of the alphabet, the derivability of graphemes from phonemes is far from being simple or invariable. In printed English, for example, neither the ampersand nor the dollar sign is speech-dependent in any direct sense.

Pithecglossics?

Perhaps the most exciting recent development in the visual language field is what might be called pithecglossics: the acquisition of manual signing, plastic token graphics, and computer console "typewriting" by apes.¹⁹ Thus far, only psychologists have been involved in such teaching and only chimpanzees and gorillas in such learning. I would be the first to cheer if the next pithecglossic teacher were to be a linguist and his next pupil an orang-utan or gibbon! Were this to happen, we might be happy witnesses not only to the breakdown of linguistic "phonocentricity" but also to an extension of the bridge-building between zoology and linguistics so eruditely begun by Thomas Sebeok.²⁰

Degrees of Phonemicity

A different type of boundary problem is presented by disputes over the phonemic status of certain phones and the phonotactic permissibility of certain phonemic sequences in various languages. Although most phonologists insist on all-or-none resolutions to such disputes, my own feeling is that phonemicity and eutaxis are themselves matters of degree rather than of kind.²¹ In English, for example, there are phonemes, such as /ɹ/ for many speakers, which become distinctive only in rapid speech; others, like /æ/, which are absent from some dialects; and still others, like /ŋ/ and /ʒ/, which can, for at least some idiolects, be more economically re-analyzed as phonemic clusters. All of them, I should say, are in some sense less phonemic than, say, the segmentals /i/, /a/, /n/, and /z/. Similarly with regard to the consonantal onset of the words beautiful, new, Lewis, and rude: occurrence of the glide /y/ decreases in frequency through the series. Yet we cannot say flatly that any one of the /Cy/ clusters is dystactic, since many speakers say /nyuw/, some /lyúwɨs/, and a few /ryuwd/.

Moving from a unilingual to a global overview of the question of phonic gradiency and discreteness, I would say, in general terms, that the relative phonemicity of speech-sound types is as follows:

1. consonants
2. vowels
3. pauses
4. accentuations

Phonographic orthographies, I think, support this seriation. For, in terms of implicational universals, I know of no orthography that represents pitches or stresses without representing vowels and consonants. Yet many represent consonants without vowels, vowels without pauses, and pauses without stress or pitch.

Grammar as Linguistic Mesoderm

I sometimes wonder if it is possible for a Lacusian to make a formal presentation of any appreciable scope without casting a ceremonial brickbat in the direction of M.I.T. Apparently such restraint can be achieved only by the exercise of iron self-discipline -- more self-discipline, in short, than I can muster. My brickbat has to do with grammar (and, within grammar, primarily with syntax) as the "deep structure" of language. My quarrel here is not with either the metaphor of linguistic space or the postulation of directional polarity within that space; I am relatively untroubled by that Transformational heresy known as Generative Semantics. My quarrel, rather, is with the notion that the stratum of language which mediates between the poles of phonology (or expression) and semantics (or meaning) can at the same time itself constitute an equivalent pole. Here I follow Alvin Liberman in supposing that "grammatical codes developed in evolution as a kind of interface" between intention and utterance²²-- presumably after those intentions grew too complex to be directly expressed by a handful of utterances. Pursuing his evolutionary image as literally as I dare, I would then characterize grammar as a communicative mesoderm. The mesoderm is defined by anatomists as a layer of tissue (consisting, among ourselves, chiefly of bone and muscle) connecting the endoderm (digestive and reproductive organs) with the ectoderm (nerves and skin). Embryologically, this layer develops during the third week of human pregnancy, as the embryo progresses from the blastula to the neurula stage -- but well before it has become an embryo sensu stricto, still less a fetus. Phylogenetically, the mesoderm probably developed during the late Proterozoic Era. At any rate, by the Cambrian Period, about 600 million years ago, molluscs and arthropods had it, though sponges did not and still do not.

The analogy to be drawn here between fauna and language is that 2-layered and 3-layered systems can and do coexist in both domains. According to Gerard Diffloth,²³ one of the characteristics of what I call allolanguage (and he calls "expressive language") is that it lacks grammar and is therefore insusceptible to transformational analysis, much as invertebrates are insusceptible to osteological analysis.

Linguistic Introspection and Linguistic Prescription

One of the advantages of presidential status, however ephemeral, is that it permits one to philosophize and even to pontificate with minimal vulnerability to the charge of irrelevance. As I look back to the Structuralist heyday during the decade following World War II, I recall how ambivalent I felt about it. For the purposes of my ethnolinguistic fieldwork, Structuralism was a breath of freedom, since the philological Traditionalism in which I had been schooled proved worse than

useless to me in the process of speech elicitation. But I was troubled then, as I still am, by the Structuralist ban on introspection and prescription. As regards the practice of using oneself as an informant, I am happy to follow my earlier brickbat with a bouquet and declare my gratitude to the Transformationalists for making it respectable to supplement external with internal linguistic data.

As regards the practice of making normative judgments about usage, however, I find nearly all contemporary schools unduly reticent. There is probably nothing that laymen want from us as much as they want guidance on usage; and it seems to me that we sometimes take an almost sadistic pleasure in flaunting our non-directiveness in respect to such questions. If social studies have shown us anything, they have demonstrated, I think, that value-free science is an illusion: the best we can do is avow our biases as openly as possible so that others may be both informed and forewarned by our avowals.²⁴ Here I have no hesitation about getting the prescriptive ball rolling by declaring, for example, my preference for the term unilingual over the term monolingual, not only because the latter term is an etymological hybrid but also because it is discordant with the sequent term bilingual (for which no one, so far as I know, either writes or proposes *dilingual).

Linguistic Comfort

Closely related to the subjects of introspection and prescription is that of what I might call linguistic comfort. Though I cannot recall ever having heard it discussed by linguistic professionals, I know that it has always concerned me, at least subliminally. And of late I have begun to think that it may hold theoretical interest. In any case, to confine myself again to phonology, I have never felt comfortable with words that contain more than two successive minimally stressed syllables. My inclination, when forced to use such words, has been to drop one of the stressless vowels if possible and, if not, to shift the primary stress to a more central position in the word. Examples are "fev' rishness" for feverishness and hallucinogen for hallúcinogen. I haven't yet plucked up the courage to substitute *attribútable for attributable; but, as my age and eccentricities increase, I expect that I will find it continually easier to substitute comfortably uncommon pronunciations for uncomfortably common ones.

The Question of Linguistic Orthogenesis

Before closing, let me add a brief programmatic note to my multidirectional ramblings. Most of us, I am sure, have wondered at one time or another why it is that a subject of abiding fascination to us awakened so little responsive interest in our colleagues. For me, such a subject -- drawn, once again, from

phonology -- is directionality in historically attested (or prehistorically reconstructed) sound change. It has struck me repeatedly, for example, how commonly velar stops shift, by way of frontal affricates, to apical fricatives but how rarely, if at all, the reverse shift occurs. Such apparent unidirectionality is of course reminiscent of what biologists call orthogenesis. A familiar example is the constant trend among horses over the past 50 million years toward greater size, more monodactylous feet, and higher-crowned teeth. What evolutionists cannot agree on is whether such long-term trends are merely artifacts of the fossil record or inherent tendencies of the biosphere. In the case of historical phonology, I would like to know how many widespread types of sound-change there are and how high the conformity to each type of "phonic orthogenesis" actually is. If any of you have made any such survey or would like to undertake one, I hope that you will let me know so that we can join forces.

References

1. George L. Trager, "The Field of Linguistics," Occasional Paper No. 1, Studies in Linguistics, Norman, Oklahoma, 1949
2. Roger W. Wescott, "Allolinguistics: Exploring the Peripheries of Speech," The Second LACUS Forum, Peter A. Reich, ed., Hornbeam Press, Columbia, S.C., 1975
3. See, for example, Dream Telepathy by Montague Ullman and Stanley Krippner, MacMillan, New York, 1973.
4. Eric P. Hamp, A Glossary of American Technical Linguistic Usage, 1925-1950, Spectrum Publishers, Utrecht, Netherlands, 1957
5. Mario Pei, A Glossary of Linguistic Terminology, Anchor Books, Doubleday and Co., Garden City, N.Y., 1966
6. William C. Stokoe, Jr., Semiotics and Human Sign Languages (vol. 21 of Approaches to Semiotics, Thomas A. Sebeok, ed.) Mouton, The Hague, Netherlands, 1972
7. R. Allen and Beatrice Gardner, "Teaching Sign-Language to a Chimpanzee," Science, vol. 165, pp. 664-672, 1969
8. Harold T. Hayes, "The Pursuit of Reason," The New York Times Sunday Book Review, pp. 21-23, June 12, 1977
9. Eugene Linden, Apes, Men, and Language, Penguin Books, New York, 1974

10. Roger W. Wescott, "Linguistic Iconism," Language, Baltimore, Md., June, 1971
11. M. Lionel Bender, "Linguistic Indeterminacy: Why You Cannot Reconstruct 'Proto-Human'," Language Sciences, August, 1973
12. Alfred L. Kroeber and Clyde Kluckhohn, Culture: A Critical Review of Concepts and Definitions, Vintage Books, Random House, New York, n.d. (originally published by the Harvard Peabody Museum in 1952)
13. H. Allen Gleason, Jr., An Introduction to Descriptive Linguistics, revised, Holt, Rinehart, and Winston, New York, 1961; pp. 6-7
14. Morris Swadesh, The Origin and Diversification of Language, Aldine Atherton, Chicago, 1971; "Vocabulary Growth and Lexical Changes," pp. 27-31
15. Susan Goldin-Meadow and Heidi Feldman, Science, vol. 197, 1977, pp. 401-403.
16. Ibid., p. 403
17. Dr. Henry Guze, Founder of the Society for the Scientific Study of Sex, New York, N.Y. (deceased 1970)
18. Op cit. (fn. 1)
19. Linden, op. cit. (fn. 9)
20. Thomas A. Sebeok, ed., Animal Communication, Indiana University Press, Bloomington, 1968
21. Roger W. Wescott, "Degrees of Phonemicity in Bini," Educational Resources Information Center, Center for Applied Linguistics, Washington, D.C., February, 1968
22. Alvin M. Liberman, "How Abstract Must a Motor Theory of Speech Perception Be?" Haskins Laboratories Status Report on Speech Research, October-December 1975, pp. 1-15 (cited by Dwight Bolinger, The Third LACUS Forum, 1976, pp. 5, 6, and 11)
23. "Notes on Expressive Meaning," Papers of the Chicago Linguistic Society, April 1972, pp. 441-445
24. Robert W. Friedrichs, A Sociology of Sociology, The Free Press, New York, 1970; Chapter 4, "The Cloak of Neutrality"

**DEVELOPMENTAL PSYCHOLINGUISTICS:
THE ACQUISITION OF LANGUAGE**

THE ACQUISITION OF CZECH IN THE ENGLISH ENVIRONMENT

Vit Bubenik

Memorial University of Newfoundland

This study reports on my observations of the development of my daughter's Czech idiolect from three to six years of age (born from Czech parents in St. John's 1971). Since Monica was simultaneously acquiring English from her play-mates in the neighborhood, in the nursery and in the kindergarten, her linguistic development was characterized by various complications typically occurring in bilingual children. I will concentrate on only a few aspects of her development, namely the acquisition of verbal categories based on relational semantic criteria (aspect and tense), as drawn on my diary records.

First of all, a few words about the grammatical structure of Czech, which belongs to the Slavic group of languages. Czech has three genders and seven cases (including the vocative); depending on the inflectional type, nouns, adjectives and pronouns have usually between 14 and 10 forms per gender. Verbs are conjugated for number and person, and in the past also for the gender of subject noun (masculine and feminine in all persons, the neuter being practically limited to the 3rd Pers, e.g. muž přišel "the man came", žena přišla "the woman came", dítě přišlo "the child came"). Verbal categories include tense (synthetic present and past, and analytic future), grammatical aspect (perfective - imperfective) and lexical aspect (determinate - indeterminate, limited basically to translocative verbs).

In Slavic pedolinguistics, the order and rate of acquisition of these morphological classes in terms of their relative conceptual difficulties have been studied most extensively for Russian by Gvozdev (1961). Their order is basically as follows: 1) number, diminutive suffixing, imperative; 2) case, tense, person; 3) conditional; 4) nominal derivational suffixes; 5) gender.

A glance at the diary for my daughter shows basically the same sequence -- with the notable exception of the conditional (Czech has two conditionals, present and past, whereas Russian has only one and its morphology is very simple). The first entries for grammatical categories appear around the age of two (in the speech of Gvozdev's son markers for number, cases and tense appeared between 1;11 and 2;0; in the speech of M. they appeared between 1;11 and 2;1). According to Gvozdev, the learning and full stabilization of Russian morphology and morphophonemics is considerably drawn-out, until the age of seven or eight years. The same is true for Czech -- as a matter of fact, we

should expect this process to be even more protracted, given the additional complications of the Czech morphology, most notably:

- i) In Russian the morphological distinction between animate and inanimate nouns is confined only to the accusative of nouns, whereas in Czech this category has expanded to other cases (Dat, Loc Sg and Nom Pl); a result of this is a considerable complexity of nominal paradigmatic relationships in Czech.
- ii) The Czech system of aspect is complicated by the expansion of the category of indeterminacy (= multiplicity). In Czech it is possible to form so-called frequentative counterparts of indeterminate verbs (against two Russian forms xodil "used to go" and prixodil "used to come", Czech has four: chodil and chodíval "...frequently", přicházel and přichážíval "...frequently").
- iii) Multitudinous diversity of verbal and nominal classes is typical of both languages; additional complications for learners arise in connection with morphophonemic pattern and accent ("free" word-stress in Russian and "free" length in Czech). It seems, however, that Czech accent (with automatic stress on the initial syllable and "free" length alternations on post-tonic syllables) causes additional problems to bilingual infantile learners -- the reason being obviously its incomparability with the English accent pattern.

Analogical formations due to the over-regularization of the verbal and nominal inflectional classes are typical of both Russian and Czech. In M.'s speech they started appearing in increasing numbers after the age of three. E.g. the imperative (making its first appearance around 2;0) appeared in a bewildering variety of analogical and contaminated forms:

/namaluvej/ "draw the picture" (3;5), StCz namaluj
/psavej/ "write" (3;8), StCz piš

Similarly, the present was exposed to the same process of analogical levelling of the polymorphous verbal classes:

/lari se menuje/~menuje/ (3;6), StCz jmenuje se Larry "his name is Larry"
/monička pi:saje/ (3;8), StCz Monička píše "Monica is writing"

An important feature of the Slavic verbal systems is the possibility of referring to a future time by the present tense of the perfective verb, e.g. StCz vypiju to "I will drink it up" (cf. the present piju to "I am drinking it"); if the aspect of the verb is imperfective the auxiliary budu has to be used (budu pít "I will drink it"). M. started using the future auxiliary budu indiscriminately for both aspects after she was three years of age:

/ti nebudeš vipít/ (3;8), StCz ty nevypiješ "you will not drink (it) up"
/monička se bude podívat/ (3;4), StCz Monička se podívá "Monica will have a look"
/ti budeš už jet/ (4;2), StCz ty už pojedeš? "Are you going to leave already?"

The correct perfective forms for future time reference appeared

around the age of four and a half years:

/ja:napsaju babiĉce dopis a babiĉka napsaje moniĉce/ (4,5), StCz já napišu babiĉce dopis a babiĉka napiše moniĉce "I will write a letter to my granny and the granny will write a letter to me". (Note her analogical formations napsaju formed on the pattern of the past napsal; similarly přiřlat (4,8), StCz přijít "to come" formed on the pattern of the past feminine přiřla "she came").

However, the wrong forms with the auxiliary are still quite common in her speech:

/ja: budu pra:sknout andrejka wiθ ðis hula hup/ (5,7), StCz (já) prášknú Andrejku tímto hula-hup "I will slash Andrea with this hoola-hoop".

Obviously, we may wonder whether her false system of the auxiliary could have been modelled on that of English (or reinforced by the English system). Similar systems, however, are common in monolingual learners of Slavic languages. The legitimate question in M.'s case is whether the perseverance of this false system is attributable to the pressure of bilingualism. It was especially in connection with verb "to be" that her auxiliary budu struck me as a replica of English "will":

/tadi to bude bit/ (4,4), StCz tady to bude "it will be here"

I still do not know the true reason for her syntactic patterns; my strong impression is that her imitation of the English auxiliary system with its rigid word-order is probably the reason why the Czech syntactic rule switching the pronominal direct object and auxiliary or predicate (when the pronominal subject is omitted) has not yet been fully acquired ($S_{Pron} O_{Pron} Aux V_{Inf} \rightarrow Aux O_{Pron} V_{Inf}$). Thus M. may model "I will send it to somebody"

on the English pattern:

/ja: pudu sendovat to do teta Jana/ (5,3), StCz já to pošlu tetě Janě (~pošlu to...) "I will send it to aunt Jane". (Here the "auxiliary" is not budu but the lexical verb /pudu/ < půjdu "I will go").

/ju: wil natří:t to a ja: nebudu škra:bat to/ (5,9), StCz ty to natřeš a já to nebudu škrábat "you will oint it and I won't scratch it".

In this interesting example M. used the English auxiliary "will" in the first coordinated clause and the Czech budu in the second one. Since the verb in the first clause is perfective the auxiliary cannot be used here. It may be noted that M.'s construction with the false auxiliary is symmetric, whereas the correct construction is asymmetric ($S O V_{Perf} + S O Aux V_{Impf}$).

Similar examples show that even today (5,11) M. still has not fully learnt that only the present tense may be used for future time reference with perfective verbs. The reason seems to be two-fold: i) her imitation of the English auxiliary system, ii) difficulties connected with the acquisition of the peculiar syntactic rule of inversion in Czech:

Perfective Verb:	já to pošlu	S O V
	↓	↓
	Ø pošlu to	Ø V O
Imperfective Verb:	já to budu škrábat	S O Aux V
	↓	↓
	Ø budu to škrábat	Ø Aux O V

At present, M. clings stubbornly to her English word-order for Czech sentences (S Aux V O) and the above rule is apparently far from being internalized. What is of particular interest is that this retardation in acquisition of syntax apparently interferes with her learning of a specifically Slavic morphological phenomenon -- that the present tense of the perfective verb refers to future time.

This perhaps is an important observation since in Slavic pedolinguistics it is maintained that children consider perfectiveness as the fundamental and imperfectiveness as the secondary aspect in verbs (Pačesová (1968:235)). To be sure, even M. manifested her predilection for the marked category of perfectiveness. E.g. double marked forms (palatal consonant and perfectivizing infix -n-) were fairly common in her speech: oblečnout (+ Colloquial oblíct) "dress up":

/dedi mu:žěš mne oblečnout/ (4,7,20), StCz tatínku, můžeš mne obléci "daddy, can you dress me up?"

The same verb appeared even without the nasal infix:

/dedi mu:žěš mne oblečet/ (4,8,3)

Here perfectiveness is marked only by the palatal consonant of the root -- compare:

	StCz	ColCz	M.'s idiolect
Imperfective	oblékat	(v)oblikat	?
Perfective	obléci	(v)oblíct	oblečnout oblečet

The preponderance of perfective forms in the lexis of monolingual Slavic children does not seem to be accidental -- apart from the fact that children like highly marked forms (witness double-diminutivized forms which appear very early), we are reminded by Pačesová (1968:236) that "the child shows a natural inclination to judge the event from the view-point of result rather than from the view-point of the action". I recorded instances when M. used the passive participle of the perfective verb in a hybrid construction with the verb míti "have" in order to emphasize the resulting state:

/ja: ma:m napršenu ruku/ (3,11), StCz napršelo mi na ruku (lit.) "I have my hand rained on" (i.e. "the rain drops fell on my hand")

However, a child growing up bilingual in any Slavic language and English will have to accomodate -- at least temporarily -- quite a few lexical borrowings from English and it is worth emphasizing that verbal roots borrowed from English become almost invariably imperfective. The Czech imperfective suffix added by M. to these roots was -ova-:

/pul+ova+t/ (4,6) "to pull"

/otšarp+ova+t/ (4,6) "to sharpen"

Of course, they can be used even to describe the perfective event:

/musíš to pulovat/ (4,6), StCz musíš to odtáhnout "you have to pull it away"

A frequently occurring problem of the acquisition of English lies in the over-generalization of the weak pattern for irregular verbs; in Czech the formation of verbal forms in the present is exposed to the analogical influence of the highly productive Class V (the type děl+ám "I am making"; the other four classes have the endings: -u, -nu, -ji, -ím). Some of the forms occurring in the speech of monolingual Czech children are listed by Pačesová (1968: 233): /pi:sa:m/, StCz píšu (Class I) "I am writing"; /spa:m/, StCz spím (Class IV) "I am asleep" etc. Similar forms in my daughter's speech appeared only sporadically and quite late:

/dedi ja: už nekašla:m/ (5,6), StCz tatínku, ja už nekašlu "daddy, I don't cough any more".

On the other hand, analogical formations such as /napsaju/ (4,5), StCz napišu "I will write" show the influence of the colloquial (and dialectal) Class III in -ju (vs. StCz -ji). Both M.'s parents use the colloquial variant of Czech as spoken in the city of Brno and do not try to offer speech models of StCz.

Peculiar formations with -m in the 1st Pers Sg of the past tense and conditional appeared in M.'s speech between 3,6 and 5,6:

/nezapomnělam/ (3,8), StCz nezapomněla jsem "I did not forget"

/ja: vidělám/ (5,3), StCz viděla jsem "I saw"

Before discussing these, a few words on the situation in colloquial Czech. The inflections for the past of the copula in standard and colloquial Czech may be surveyed in the following chart:

		Standard Czech	Colloquial Czech
Masc Sg	1	byl jsem "I was"	byl sem
	2	byl jsi	byls
	3	byl	byl
Fem Sg	1	byla jsem	byla sem
	2	byla jsi	bylas
	3	byla	byla
Masc Pl	1	byli jsme	byli sme
	2	byli jste	byli ste
	3	byli	byli

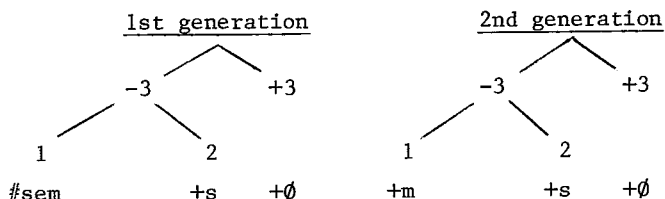
It may be noted that the situation in StCz is anomalous since the auxiliary "be" of the past appears in the 1st and 2nd Pers of both numbers but not in the 3rd Pers. ("Anomalous" must be understood in a comparative sense -- Russian lost the auxiliary in all persons, whereas Serbo-Croatian keeps the auxiliary in all persons).¹ In colloquial Czech the auxiliary is losing its identity as a grammatical word and is recategorized as a person marker. On phonological grounds we may note that the simplification of the initial cluster /js-/ → /s-/ entailed the loss of word boundary in the 2nd Sg -- +s in byls is not an auxiliary anymore but merely

a marker of the 2nd Pers. Thus even the system of colloquial Czech could be described as anomalous -- whereas the auxiliary of StCz is separated from the l-participle by a word-boundary /bil#jsem/, /bil#jsi/, there is only a morpheme-boundary in the 2nd Pers Sg in colloquial /bil#sem/, bil+s/.

The strategy employed by M. in learning this system can best be described as non-proportional analogy:

	Colloquial Czech	M.'s idiolect
Fem Sg 1	psala#sem "I wrote"	psala+m
2	psala+s	psala+s
3	psala+∅	psala+∅

The starting point for M.'s deductive innovation² could be syntactic juxtapositions of the following type: (M.'s mother) Chytla sem to. Vidělas to? "I caught it. Did you see it?". These were reanalyzed by M. as: Chytlam to. Vidělas to? In other words, M.'s paradigm was deduced from her mother's paradigm by the following "mental operation":



Whereas the 1st generation used the copula in the 1st Pers Sg as a full fledged grammatical auxiliary, the second generation reduces sem to mere +m by non-proportional analogy with the 2nd Sg +s and achieves thus a uniformity in the paradigm. Innovations of this type gave rise to Mańczak's observation (1963:37) -- expressed by him as a "law" regarding analogical innovations -- that the desinence of the 3rd Sg is less subject to analogical change than that of other persons (i.e. the desinences of the 1st or 2nd Pers are more often replaced or modified than the 3rd Pers).³

After 5,6 M. "discovered" that this restructuring of her mother's grammar was wrong and there appeared auxiliary-less "Russian-like" constructions in the 1st Pers Sg:

/ja: dala/ (5,8), StCz já jsem dala "I gave"

/ja: oblejzala to/ (5,9), StCz já jsem to oblízala "I licked it off"

The point to be made in this connection is that M.'s analogizing resulted in symmetry and greater simplicity only in the feminine paradigm; such analogizing would be impossible in the masculine. Psál sem "I wrote" (Masc) cannot be replaced by *psál+m -- even if there is the 2nd Pers psál+s -- since the cluster /-lm/ does not occur finally in native Czech words. In other words, M.'s "experiment" could not have been carried out by a Czech male child.

At this point, I would like to mention some interesting

phylogenetic parallels of these ontogenetic processes. It is a well-known fact that children introduce many analogical formations (of both proportional and non-proportional nature) -- the moot problem is, however, how much their analogizing contributes to evolutive changes in morphology. Or, more generally, what exactly is the share of infantile individuals in evolutive linguistic change? A conceptual analogy used by M. in her learning of the inflections of the past in Czech seems to be responsible for the shape of the corresponding paradigm in standard Polish. We may survey these forms in the following chart:

	Standard Czech	Standard Polish
Masc Sg 1	psal jsem "I wrote"	pisałem
2	psal jsi	pisałeś
3	psal	pisał
Fem Sg 1	psala jsem	pisałam
2	psala jsi	pisałaś
3	psala	pisała
Masc Pl 1	psali jsme	pisaliśmy
2	psali jste	pisaliście
3	psali	pisali

In contrast to the other Slavic languages, Polish has a past with personal endings which were derived from the old form of the present used as an auxiliary (as in Modern Czech). These endings were added to the past participle in -ł and in the case of the masculine a connecting vowel -e- was inserted. We may hypothesize that a starting point of this morphological change in Polish could have been "experiments" of female children in the 16th c. They even "seduced" males -- witness masculine forms pisał-e-m and pisał-e-ś with an intrusive -e- introduced probably analogically with feminine counterparts (another explanation, however, may be proposed -- intrusive -e- may have resulted through phonemicization by the following generation of the phonetic schwa which appeared between dark /ɨ/ and /m/: [pisaɨ⁹m] → /pisałem/). In any case, these innovations acquired permanence by being accepted by a homogenous Polish-speaking community at a certain point in its history. Even the verb być "be", which was the source of this innovation, has to be inflected by means of these endings (they are added to what used to be the form of the 3rd Pers Sg: jest-em "I am", jest-eś "you are" etc.). It is worth mentioning that in Polish these endings are still partly felt to be a grammatical auxiliary since they can be attached to other words such as pronouns, adverbs, adjectives and nouns: był-em ~ jam był "I was", rad-em ~ jam rad "I am glad", kiedy pisałem ~ kiedy pisał "when I was writing". It may also be noted that the 1st and 2nd Pers Pl are accented on the antepenult -- in other words, /pi'sališmy/ is still felt as consisting of a lexical word and grammatical auxiliary (not ending) despite its orthographic representation.

The situation in colloquial Czech is similar to a certain extent, namely, in the 2nd Sg: bylas ~ tys byla "you were" (Fem).

Elsewhere the auxiliary becomes an enclitic, e.g. doma ste to udělali /doma+ste#to#udělali/ "did you make it at home?". This fact, however, does not find expression in the orthography of Czech, whereas it does in Polish: W domuście to zrobili?

Returning to M.'s "Polish-like" formations with -m in the 1st Sg, we may conclude that these represent a natural result of trends in colloquial Czech. This result, of course, is by no means necessary -- thus two of M.'s Czech peers growing up bilingual in a comparable environment never created similar forms and M. herself dropped them from her intermediate grammar.

Finally, a few words on M.'s acquisition of the conditional. At present (5,11) M. still has not acquired the form of the 1st Pers Sg with a special morpheme /-x/. It should be noted that the morphology of the Czech conditional is quite complicated compared with that of Russian (which uses only the marker by with the 1-participle) or Polish. The specific endings of the standard Czech conditional (/x/ in the 1st Pers Sg and /xom/ in the 1st Pers Pl) tend to be replaced in colloquial speech by the auxiliary taken from the past:

	Standard Czech	Colloquial Czech	Standard Polish
Cond Sg 1	psala bych (Fem)	psala bysem	pisałabym
2	psala bys	psala bys	pisałabyś
3	psala by	psala by	pisałaby
Pl 1	psaly bychom	psaly bysme	pisałybyśmy

Strangely enough, in this case the model provided by M.'s parents is the StCz form bych (not bysem, which obviously by its being disyllabic destroys the symmetry of the colloquial paradigm). However, M. simply and consistently used her "Polish-like" 1-participle with -m (= recategorized auxiliary sem) with the marker of conditional by. The very first entry in my diary is an example of the conditional used in a subordinate clause of purpose:

/musim papat abi bilam silna:/ (4,4) StCz musím papat abych byla silná "I have to eat to be strong"

Again it might be of interest to point out a phylogenetic parallel to this ontogenetic stage with its "refusal" of the peculiar morpheme /-x/. Historically, the auxiliary of the conditional is the Old Czech aorist. In other Slavic languages (with the notable exception of southern group) aorist forms were lost in mediaeval times (15th c. in Russian, 16th c. in Polish). Modern Polish forms the 1st Pers of both numbers with the same endings as the past, and Russian has no auxiliary as in the past. Thus the situation in the 1st Pers Cond in StCz is anomalous and open to analogizing with the past. This is exactly what happens in colloquial Czech and in M.'s idiolect:

	Past (Sg 1 Fem)	Conditional (Sg 1 Fem)
Standard Czech	psala jsem	psala by+ch
Colloquial Czech	psala sem	psala by+sem
M.'s idiolect	psala+m	psala+m by
Polish	pisała+m	pisała+by+m

At present (5,11), M. is in the stage when her second

language dominates over the native Czech. She is aware of her bilingualism and freely translates messages in both directions. In "technical" matters (when she talks about the school, her homework etc.) she definitely prefers English. I should say common Newfoundland English abounding with "We was jumping", "She don't love me" etc. as picked up from her play-mates (this is the code she prefers to her parent's unnatural non-native language). Problems in her English are apparently identical with those of her English peers: "Could you take off this" (5,9) -- syntactic rule; "How did you done it?" (5,9) -- overmarking for tense; "Stand 'er up" (5,10) -- causative; etc. However, my overall impression is that her acquisition of Czech is considerably delayed; she still has to go a long way to master fully all the conjugational and declensional suffixes and categories and syntactic rules of Czech.

Notes

1.

	<u>Czech</u>	<u>Russian</u>	<u>Serbo-Croatian</u>
Sg 1	byl jsem "I was"	ja byl	bio sam
2	byl jsi	ty byl	bio si
3	byl	on byl	bio je
Pl 1	byli jsme	my byli	bili smo
2	byli jste	vy byli	bili ste
3	byli	oni byli	bili su
2. Defined by Andersen (1973).
3. Mańczak (1963:36):
"Quant aux catégories suivantes: singulier -- autres nombres /XIII/, troisième personne -- autres personnes /XIV/, indicatif -- autres modes /VI/,... les radicaux et les désinences des premières catégories subissent moins de changements analogiques /c'est-à-dire qu'ils sont moins souvent remplacés ou refaits/ que les radicaux et les désinences des dernières catégories."

Bibliography

- Andersen, H. (1973). 'Abductive and deductive change'.
Language Vol. 49, No. 4, pp. 765-793.
- Bar-Adon, A. (1964). "Analogy" and Analogic Change as Reflected in Contemporary Hebrew. Horace G. Lunt, ed., *Proceedings of the Ninth International Congress of Linguistics*. The Hague: Mouton & Co., pp. 758-63.
- Baron, N.S. (1973/1976). *Diachrony and Child Language. Phylogenetic Reflections of Ontogenetic Processes*. Reproduced by the Indiana University Linguistics Club, August 1976.

- Geodakjan, I.M. (1976). 'Expression of the Category of Time in the Speech of a Young Child'. J. Průcha, ed., *Soviet Studies in Language and Language Behavior*. North-Holland Publishing Company, pp. 185-8.
- Gvozdev, A.N. (1961). *Voprosy izucheniya detskoy rechi* (Questions of the Study of Child Speech). Moscow: Akad. Pedag. Nauk RSFSR.
- Kessler, C. (1971). *The Acquisition of Syntax in Bilingual Children*. Georgetown University Press: Washington, D.C. 20007.
- Mańczak, W. (1963). 'Tendences générales du développement morphologique'. *Lingua* 12, pp. 19-38.
- Pačesová, J. (1968). *The Development of Vocabulary in the Child*. Opera Universitatis Purkynianae Brunensis 129: Brno.
- Raven, R. (1968). 'Language Acquisition in a Second Language Environment'. *IRAL*, Vol. VI/2.
- Slobin, D.I. (1965). 'Grammatical Development in Russian-Speaking Children'. A. Bar-Adon and W.F. Leopold, eds., *Child Language*. Prentice-Hall 1971, pp. 344-8.
- (1966). 'The Acquisition of Russian as a Native Language'. F. Smith and G.A. Miller, eds., *The Genesis of Language*. Cambridge: M.I.T. Press, 1966, pp. 129-48.
- Vašek, A. (1976). 'On the Problem of the Isolated Language'. *Folia Lingusitica* IX-1-4, pp. 85-124.

METALINGUISTIC DEVELOPMENT OF CHILDREN IN BILINGUAL
EDUCATION PROGRAMS:
DATA FROM IRISH AND CANADIAN UKRAINIAN-ENGLISH PROGRAMS

James Cummins
The University of Alberta

By "metalinguistic development" I am referring both to the development of children's awareness of certain properties of language and also to their ability to analyze linguistic input, i.e., to make the language forms themselves the objects of focal attention and to look at language rather than through it to the intended meaning. Early investigators of children's language development have speculated that access to two languages in early childhood might promote an awareness of linguistic operations and a more analytic orientation to linguistic input. Vygotsky (1935, 1962), for example, argued that being able to express the same thought in different languages will enable the child to "see his language as one particular system among many, to view its phenomena under more general categories, and this leads to awareness of his linguistic operations" (1962, p.110). In an earlier paper directly concerned with multilingualism in children, Vygotsky (1935) suggested that when the application of sound pedagogical principles ensured that each language had an independent sphere of influence, bilingualism could orient the child towards more abstract thought processes "from the prison of concrete language forms and phenomena." Leopold (1949) and Imedadze (1960) have both argued on the basis of observational studies of children's simultaneous acquisition of two languages, that bilingualism can accelerate the separation of name and object and can focus the child's attention on certain aspects of language. In more recent years, Lambert and Tucker (1972) suggested that the experimental group in the St. Lambert bilingual education project had learned to engage in a form of contrastive linguistics by comparing similarities and differences in their two languages.

The hypotheses advanced by these investigators have recently found empirical support in well controlled studies conducted by Ianco-Worrall (1972) and Ben-Zeev (1977, 1978). Ianco-Worrall reported that Africaans-English bilingual children brought up in a one-person, one-language home environment were more oriented to the semantic rather than the acoustic properties of words and were more aware of the arbitrary assignment of words to referents than were unilingual children. In studies conducted with middle-class Hebrew-English and lower-class Spanish-English bilinguals, Ben-Zeev (1977, 1978) has reported findings which suggest that bilinguals develop a more analytic orientation to language and more sensitivity to feedback cues. Ben-Zeev (1976) hypothesizes that bilinguals develop this analytic strategy towards language

as a means of overcoming interlingual interference.

The present studies were designed to further investigate the influence of bilingualism on children's orientation to language. The first study was carried out in Ireland with English-Irish bilinguals and the second was carried out in Canada with English-Ukrainian bilinguals. Although very different at first sight the bilingual learning situations in both studies were similar in that neither Irish nor Ukrainian is a language of worldwide prestige and the motivation for learning the language is essentially ethnic and cultural.

STUDY I

Subjects

The subjects were 80 grade 3 children and 26 grade 6 children selected from four middle-class schools in Dublin, Ireland. In two of the schools all subjects were taught through Irish (a second language for the majority of pupils) while in the other two schools subjects were taught through English (pupils' home language). Two criteria were used to select the bilingual sample from the Irish medium schools: (a) that at least some Irish be spoken in the children's homes; (b) that teachers rated children at least 3 on a 5-point scale of Irish expressive skills. A rating of 3 signified that a child was able to express himself through Irish with little hesitation, though not always with grammatical correctness. Forty grade 3 children from the two Irish medium schools met these criteria and these children were matched on IQ, SES, sex and age with 40 grade 3 children from the English medium schools. Although pupils in the English medium schools were taught Irish as a school subject, their fluency in Irish was negligible. There were 20 boys and 20 girls in each group. The mean age of the unilingual group was 8 years 11 months and that of the bilingual group 8 years 9 months. The mean standard score of the bilingual group on the Otis-Lennon IQ Test (Irish version) was 113.79 (S.D. 9.67) and that of the unilingual group 113.54 (S.D. 9.57).

Sixth grade children in one of the schools were unavailable for testing and consequently sixth grade children from just one of the Irish medium and one of the English medium schools were tested. Thirteen children from the Irish medium school who met the selection criteria outlined above were matched on IQ, SES, sex and age with 13 English medium pupils. There were seven girls and six boys in each group. The mean age of the unilingual group was 11 years 9 months and that of the bilingual group 11 years 11 months. The mean standard score of the bilingual group on the Drumcondra Verbal Reasoning Test was 120.54 (S.D. 10.74) and that of the unilingual group 120.85 (S.D. 11.22).

Instruments

The tasks used in the present study were adapted from those developed by Osherson and Markman (1975) to test the hypothesis that the ability to examine language in an objective manner, apart from objects and events to which it refers, is necessary, but not sufficient, for correct evaluation of contradictory and tautological statements. These statements are termed "nonempirical" since they are true or false by virtue of their linguistic form rather than deriving their truth-value from any extra-linguistic states of affairs.

Osherson and Markman devised three tasks to measure an "objective" orientation to language and these tasks as well as a series of contradictory and tautological items were administered to the bilingual and unilingual subjects. However, analysis of the results of the "language objectivity" tasks showed that, contrary to Osherson and Markman's claims, they did not represent a unitary dimension and thus were of uncertain construct validity. Thus, only the results of the contradictory and tautological (C & T) items and that part of the Arbitrariness of Language (AL) measure which showed consistent significant correlations with both the C & T items and the other language objectivity items will be outlined.

The AL task assessed children's awareness of the fact that words are arbitrarily assigned to referents. Children were asked "Suppose you were making up names for things could you then call the sun 'the moon' and the moon 'the sun'?" and were required to justify their response. A similar measure was adapted from Vygotsky (1962) by Ianco-Worrall (1972) but in Ianco-Worrall's study children were not required to justify their responses.

The materials for the C & T task were small plastic counters or poker chips of assorted colours, which, at the start of the session, were placed on a table between the experimenter and subject. The experimenter said "Do you see these counters on the table here? Now, I am going to pick up some of the counters and say some things about the counter in my hand. Each time I do this I want you to tell me whether what I have said is true, false or whether you can't tell. Only say 'true' or 'false' if you are sure what I have said is true or false. Say 'can't tell' if it's not possible to know whether what I have said is true or false. Don't guess, and remember, pay attention to what I'm saying." The counters were then placed in a box which was put under the table out of the subject's sight.

After the subject had given his response to each item he was asked why he thought so and all justifications were recorded by the experimenter. The seven nonempirical items consisted of four contradictions and three tautologies. For three of the contra-

dictory statements and two of the tautologies the counter was hidden from the child's view in the experimenter's hand. The seven nonempirical items were followed by four empirical items which were included in order that the relationship between empirical and nonempirical items could be assessed. The items, together with the colour of the counter, if visible, and the correct answer, are shown in Table 2. The items were presented to all subjects in the order shown in Table 2.

Results

Arbitrariness of Language. Three categories of response justification were clearly distinguishable on the AL task. The most primitive of these were "empirical" justifications in which the subject invoked empirical attributes of the objects to justify his response. For example, some children maintained that the names could be interchanged because "both the sun and the moon shine", while others maintained that the names could not be interchanged "because the sun shines in the day and the moon shines in the night." The second category consisted of "rigid conventional" justifications such as "They are their right names so you couldn't change them" or "God gave things their names and people can't change them." The third category of justifications were those that recognized the arbitrary assignment of names to referents ("You could change the names because it doesn't matter what things are called"). Also included in this third category in the present analysis were responses which held that the names could not be interchanged "because people would get confused." This response was considered legitimate (and scored correct) even though it does not explicitly recognize the arbitrary assignment of names to referents. One bilingual and two unilingual children at the grade 3 level and two bilingual grade 6 children gave this response. When questioned further these children invariably stated that in principle the names could be interchanged if everyone agreed. Six children at the grade 3 level, three from each group were unable to justify their response. For purposes of analysis, these children were classified in the "empirical" category.

The distribution of bilingual and unilingual responses to the items in the AL task is shown in Table 1. Chi-square analyses showed significant differences at both grade levels (grade 3; $\chi^2 = 7.40$, $df = 2$, $p = .02$; grade 6 (empirical and rigid conventional categories collapsed): $\chi^2 = 5.85$; $df = 1$, $p < .02$). This finding is consistent with, and extends Ianco-Worrall's since justifications rather than actual responses (yes, no) were analyzed.

Table 1

Percentage of Bilingual and Unilingual Children in
Each Category on Arbitrariness of Language Task

	<u>Grade 3</u>		<u>Grade 6</u>	
	<u>Bilingual (N=40)</u>	<u>Unilingual (N=40)</u>	<u>Bilingual (N=13)</u>	<u>Unilingual (N=13)</u>
Empirical	57.5 (23)	57.5 (23)	7.7 (1)	46.2 (6)
Rigid Conventional	2.5 (1)	20.0 (8)	7.7 (1)	15.4 (2)
Arbitrary	40.0 (16)	22.5 (9)	84.6 (11)	38.5 (5)

Contradictions and Tautologies. The empirical (items 8-11) and nonempirical (items 1-7) questions and the percentages of bilingual and unilingual children correct at each grade level are shown in Table 2. At the grade 3 level there are significant differences between bilingual and unilingual groups on contradictory items 3 and 6 ($\chi^2 = 4.02$, $df = 1$, $p < .05$), and differences between the groups on the two other contradictory items (2, 6) approach significance (item 2: $\chi^2 = 3.66$, $df = 1$, $p < .06$; item 6: $\chi^2 = 2.58$, $df = 1$, $p < .11$). At least twice as many bilingual children as unilingual children were correct on each of the four contradictory items. Differences between bilingual and unilingual groups in the total number of nonempirical items correct were significant ($F = 4.62$, $df = 1$, 78 , $p < .05$).

On the empirical questions, differences between the groups are nonsignificant on items 8, 9 and 10 but the unilingual group performs significantly better on item 11 ($\chi^2 = 4.71$, $df = 1$, $p < .05$). The inferior performance of the bilingual group on items 10 and 11 can be attributed to failure to shift strategies between the "hidden" nonempirical items and the "hidden" empirical items (10 and 11). Many of the bilingual children were just beginning to grapple with the reasoning processes involved in solving the nonempirical items and their application of this strategy was still relatively inflexible. A larger number of the unilingual group did not have the problem of shifting strategies since they had applied an inappropriate empirical strategy to the nonempirical items.

At the grade 6 level, there is also evidence of bilingual superiority on the "hidden" nonempirical items. Between group differences are significant on item 2 ($\chi^2 = 4.25$, $df = 1$, $p < .05$) and approach significance on items 1 and 5 (item 1: $\chi^2 = 3.39$, $df = 1$, $p < .07$; item 5: $\chi^2 = 2.60$, $df = 1$, $p < .11$). There were no differences between the groups on the "visible" items 6 and 7. The fact that the counter was visible (thereby

Table 2
Performance of Bilingual and Unilingual Groups on Nonempirical and Empirical Questions

	Grade 3			Grade 6		
	Bilingual	Unilingual	Total %	Bilingual	Unilingual	Total %
Nonempirical:						
1. Either the counter in my hand is not white or it is white (H, T) ^a	0.0	0.0	0.0	23.1 (3)	0.0	11.5
2. The counter in my hand is blue and it is not blue (H, F)	30.0 (12)	12.5 (5)	21.2	53.8 (7)	15.4 (2) *	34.6
3. The counter in my hand is not white and it is white (H, F)	27.5 (11)	10.0 (4) *	18.8	53.8 (7)	30.8 (4)	42.3
4. Either the counter in my hand is yellow or it is not yellow (H, T)	0.0	3.0	0.0	23.1 (3)	15.4 (2)	19.2
5. The counter in my hand is both blue and it is not blue (H, F)	30.0 (12)	15.0 (6)	22.5	53.8 (7)	23.1 (3)	38.5
6. The counter in my hand is green and it is not green (V, F, green)	27.5 (11)	10.0 (4) *	18.8	46.2 (6)	38.5 (5)	42.3
7. Either the counter in my hand is red or it is not red (V, T, blue)	10.0 (4)	7.5 (3)	8.7	38.5 (5)	23.1 (3)	30.8
Mean number correct nonempirical	1.25	.55 *	.90	2.9	1.5	2.2
Empirical:						
8. The counter in my hand is either not yellow or it is blue (V, T, blue)	85.0 (34)	80.0 (32)	82.5	100 (13)	61.5 (8) *	80.8
9. The counter in my hand is yellow and it is not green (V, F, green)	87.5 (35)	77.5 (31)	82.5	100 (13)	92.3 (12)	96.2
10. The counter in my hand is blue and it is not yellow (H, CT)	27.5 (11)	37.5 (15)	32.5	92.3 (12)	53.8 (7) *	73.1
11. The counter in my hand is either red or it is not green (H, CT)	20.0 (8)	42.5 (17) *	31.3	92.3 (12)	53.8 (7) *	73.1
Mean number correct empirical	2.38	2.20	2.29	3.80	2.70 *	3.25

* $p < .05$

^a The first letter in brackets after each item signifies the status of the counter, whether hidden (H) or visible (V). The second letter signifies the correct answer, either true (T), false (F) or can't tell (CT). For the visible items the colour of the counter is also shown.

ruling out a "can't tell" response) and the practice effect of items 1 - 5 seems to have improved the performance of the unilingual children on these items. This suggests that the unilingual children are as capable of the appropriate reasoning as the bilingual children (which they are, since groups are matched on verbal reasoning scores) but they analyze linguistic input less closely, being more content to give the obvious "can't tell" response to the "hidden" nonempirical items 1 - 5. Differences between bilingual and unilingual groups in the total number of nonempirical items correct did not attain statistical significance ($F = 2.33$, $df = 1, 24$, $p < .14$).

The performance of the bilingual and unilingual grade 6 children on the empirical items is quite revealing. There are significant differences between the groups on three of the four items (item 8: $\chi^2 = 6.19$, $df = 1$, $p = .01$; items 10 and 11: $\chi^2 = 4.89$, $df = 1$, $p < .05$) and the difference in total number of empirical items correct is significant ($F = 11.25$, $df = 1, 24$, $p < .01$). It is clear that the unilingual children have difficulty in switching strategy between the nonempirical and empirical items. Only 61.5% of this group are correct on the first empirical question, considerably less than the grade 3 unilinguals. They recover on the second empirical question but many again fail to shift strategy on items 10 and 11. There is little doubt that the vast majority of the grade 6 unilingual children would have solved the empirical problems had these problems been presented on their own. Their relative inferiority on these problems under the conditions of the present study is attributable, therefore, not to lack of reasoning ability but to a less flexible and analytic orientation to linguistic input.

STUDY II

The second study was designed to investigate whether bilinguals' superior ability to analyze linguistic input resulted from the process of acquiring two languages or, alternatively, was it a result of actually functioning fluently in two languages over a prolonged period of time.

Subjects

The subjects were selected from grade 1 and 3 classes in four of Edmonton Public School System schools where a Ukrainian-English bilingual program was offered. From the bilingual classes in these schools a group of relatively fluent bilinguals was selected on the basis of the extent of Ukrainian usage in the home and teachers' ratings of Ukrainian speaking skills. Children who spoke Ukrainian at least half the time to one parent or who spoke some Ukrainian to both parents and who were rated at least 3 by their teachers on a 5-point scale of Ukrainian speaking skills were eligible for selection. Twelve grade 3 children from three bilingual classes met both these criteria. At the grade

1 level it was necessary to select children from four bilingual classes in order to find 12 who met both criteria.

This fluent bilingual (FB) group was matched on IQ (Raven's Progressive Matrices), SES, sex and age with a non-fluent bilingual (NFB) group (i.e. children in the same classes who had little or no Ukrainian at home and whose Ukrainian speaking skills were rated less than 3) and a unilingual (U) group attending regular classes in the same schools.

Instruments

1. Semantic-Phonetic (SP) Preference. This task was developed by Purbhoo and Shapson (1975) on the basis of Ianco-Worall's task. Subjects were asked 16 questions of the type: "Which word is more like ARM, is it HAND or is it ART?" Subjects who made at least 12 semantic choices were scored 1 and those who made less than 12 were scored 0.

2. Verbal Transformation Effect (VTE). In this test, described by Warren & Warren (1966) a stimulus word is repeated twice a second on tape and subjects typically experience an illusion that the stimulus word is constantly changing. Ben-Zeev (1977, 1978) reported that both Hebrew-English and Spanish-English bilingual children experienced significantly more changes in the VTE than their unilingual controls. She speculates that these differences may be due to increased processing effort on the part of the bilinguals. However, there is as yet little evidence to support this interpretation of the VTE.

3. Arbitrariness of Language (AL). This test was an extended version of the one used in Study I involving three items (sun-moon, cat-dog, chair-book) rather than just one. The same scoring criteria were used as in Study I.

4. Class Inclusion (CI). This Piagetian test consisted of two parts with four items in each. In the first part the child was shown line drawings of two groups of objects each of which formed a subclass of a larger superordinate class. There were four objects in one subclass and two in the other and subclasses were spatially separated. Children were asked questions of the type "In this picture are there more spoons or more things to eat with?" The second part consisted of four questions of the type "In the whole world are there more animals or more cows?" Children were required to justify their responses on the second part and only justifications which recognized the class inclusion relationship ("more animals because cows are animals") were credited. Responses to both parts were combined giving a maximum score of eight.

This task was included, not as a measure of reasoning, but as a potential index of a child's ability to analyze linguistic

input. As Jonckheere (1975) points out "linguistic competence goes hand in hand with the operation of class inclusion, since it is not easy to see how one can understand the question and yet not be able to give the correct answer." (p.168). Hodkin (1977) has recently shown that the form of the typical class inclusion question presents considerable difficulties to the child. It was precisely because of these difficulties, which require analysis of language in addition to reasoning, that the class inclusion task was included in the present study.

5. Ambiguities (Amb). This 12-item test was taken directly from Kessel (1970) and assessed children's sensitivity to lexical (L), surface structure (SS) and underlying structure (US) ambiguities. The subject was shown four line drawings, two of which depicted different meanings of a sentence which was read aloud by the experimenter. Examples of each of the types of ambiguous sentences are: L - "The boy picked up the bat" (baseball bat, flying bat); SS - "He told her baby stories". (He told her baby-stories, He told her baby stories); US - "The eating of the chicken was sloppy" (chicken eating sloppily, girl and boy eating chicken sloppily). Children were scored 2 if they gave the two meanings and correct justifications spontaneously, 1 if they gave the two meanings and justifications after probing and 0 if they chose the wrong pictures or only one correct picture or if their justifications showed they had not grasped both meanings. The probing procedure for each item is described in Kessel (1970).

Results

The results for the VTE and AL tasks can be summed up quite simply by saying that there were no group differences at either grade level on these tasks. Thus, the findings of superior performance by bilingual children on AL in Study I and on VTE in Ben-Zeev's studies were not replicated.

There were significant group differences at the grade 1 level on the SP preference task. Nine of the U group, seven of the FB group and only four of the NFB group made use of a semantic strategy. A Chi-square test revealed a significant difference between U and NFB groups ($\chi^2 = 4.20$, $df = 1$, $p < .05$). However, the difference disappears by grade 3 where at least ten out of 12 children in each group utilized a semantic strategy. The grade 1 difference is probably due to the necessity to "train one's ear" and pay attention to phonetic similarities and dissimilarities in the initial stages of the bilingual program. The lack of differences between FB and U groups on this task is not consistent with Ianco-Worrall's findings. However, her 4 - 6 year old sample was considerably younger than the present grade 1 sample and the bilingual learning situation was also quite different.

These findings do not support the hypotheses that bilinguals are more semantically oriented and have a greater awareness of

the arbitrary nature of word-referent relationships. However, the results of the Class Inclusion and Ambiguities tests are consistent with previous findings in that they suggest that bilingualism promotes an analytic orientation to linguistic input.

The CI task proved difficult for a large majority of grade 1 children and no group differences were found. However, at the grade 3 level differences between FB and U groups approached significance (FB $\bar{X}$ = 3.83, U $\bar{X}$ = 1.92, t = 1.75, p < .10 two-tailed, p < .05 one-tailed). The NFB mean was intermediate between the other two groups. Since the FB and U groups have been matched on general reasoning ability it seems reasonable to suggest that the trend for bilingual superiority on the CI task is related to the other hypothesized component of CI performance - analysis of language.

The Ambiguities findings presented in Table 3 are consistent with this interpretation. Three-by-two analyses of variance showed significant main effects for Language Group on the Surface Structure, Underlying Structure and Total Ambiguities scores.

Table 3

Means of Fluent Bilingual, Nonfluent Bilingual and Unilingual Groups on Ambiguities Task

		Grade 1	Grade 3	Language Group Main Effect
Amb - L	FB	6.3	7.5	F = 1.65
	NFB	5.3	7.5	
	U	5.1	7.1	
Amb - SS	FB	2.5	2.9	F = 6.03**
	NFB	1.1	2.1	
	U	0.9	1.9	
Amb - US	FB	3.8	6.4	F = 9.61**
	NFB	1.7	4.3	
	U	0.5	4.8	
Amb - Total	FB	12.6	16.8	F = 10.16**
	NFB	8.1	13.8	
	U	6.5	13.8	

** p < .001

Scheffé multiple comparisons of main effects showed no group differences on lexical ambiguities, significant differences between FB and NFB and between FB and U on surface structure ambiguities (FB vs. NFB, $p < .03$; FB vs. U, $p < .01$), underlying structure ambiguities (FB vs. NFB, $p < .01$; FB vs. U, $p < .001$) and total ambiguities score (FB vs. NFB, $p < .01$; FB vs. U, $p < .001$). None of the NFB vs. U comparisons were significant.

Conclusions

The findings of the first study suggested that bilingualism might promote both metalinguistic awareness and an analytic orientation to linguistic input. The second study did not support the hypothesis that bilingualism promotes the development of metalinguistic awareness but there was again evidence that bilingual children are better able to analyze linguistic input. In the second study the fact that the performance of the non-fluent bilingual group was similar to that of the unilingual group suggests that this analytic orientation to language is a result of actually functioning fluently in two languages rather than the language learning experience per se.

Footnote

I would like to sincerely thank all the teachers and pupils in the schools which participated in both studies. The assistance of Dale Lamont of the Edmonton Public School System throughout the second study was particularly valuable.

References

- Ben-Zeev, S. Mechanisms by which childhood bilingualism affects understanding of language and cognitive structures. Paper presented to Bilingualism Conference, Plattsburgh, N.Y., Spring, 1976.
- Ben-Zeev, S. The influence of bilingualism on cognitive development and cognitive strategy. Child Development, 1977 (in press).
- Ben-Zeev, S. The effect of Spanish-English bilingualism in children from less privileged neighborhoods on cognitive development and cognitive strategy. Working Papers in Bilingualism, 1978 (in press).
- Hodkin, B. Linguistic factors in the assessment of logical development. Paper presented to the annual conference of the Canadian Psychological Association, Vancouver, 1977.
- Ianco-Worrall, A. Bilingualism and cognitive development. Child Development, 1972, 43, 1390-1400.

- Imedadze, N.V. K psikhologicheskoy prirode rannego dvuyazyehiya (On the psychological nature of early bilingualism) Voprosy Psikhologii, 1960, 6, 60-68.
- Jonckheere, A.R. Commentary. In N. O'Connor (Ed.) Language, cognitive deficits and retardation. London: Butterworths, 1975.
- Kessel, F. The role of syntax in children's comprehension from ages six to twelve. Monographs of the Society for Research in Child Development, 35, 1970.
- Lambert, W.E., & Tucker, G.R. Bilingual education of children: The St. Lambert experiment. Rowley: Newbury House, 1972.
- Leopold, W.F. Speech development of a bilingual child. Vol. 3. Evanston: Northwestern University Press, 1949.
- Osherson, D.E. & Markman, E. Language and the ability to evaluate contradictions and tautologies. Cognition, 1975 3, 213-226.
- Purbhoo, M. & Shapson, S. Transition from Italian. Toronto: Toronto Board of Education, 1975.
- Vygotsky, L.S. Multilingualism in children. Translated by Metro Gulutsan and Irene Arki, Centre for East European and Soviet Studies, The University of Alberta, Mimeo, 1975. The essay appears in a collection of essays written by Vygotsky and edited by L.V. Zankov, Zh. I. Shif and D.B. El konin, Umstvennoe razvitie detei v protsesse obucheniia, spornik statei (Mental development of children in the process of education, a collection of essays). Moscow and Leningrad: State Pedagogical Publishing House, 1935.
- Vygotsky, L.S. Thought and language. Cambridge: M.I.T. Press, 1962.
- Warren, R.M., & Warren, R.P. A comparison of speech perception in childhood, maturity and old age by means of the verbal transformation effect. Journal of Verbal Learning and Verbal Behaviour, 1966, 5, 142-146.

QUESTION FORM AND QUESTION FUNCTION IN THE SPEECH OF YOUNG FRENCH L2 LEARNERS

Patsy M. Lightbown
Concordia University

In this study, the development of certain question forms and question functions was traced in the speech of two young boys acquiring French as a second language (L2). Previous research on L2 acquisition has shown that, because the L2 speaker's cognitive maturity is much greater than his linguistic ability, he must use a small number of linguistic forms to serve a larger number of semantic and pragmatic functions (Felix, 1975; Lightbown, 1977a, b; Wagner-Gough, 1975). This study was designed to explore this language acquisition strategy in the development of question forms and question functions. In addition, the sequence of the emergence of question forms and functions in L2 speech was compared to that observed in L1 development.

The study reported here focuses on question production rather than on response to questions. Further, this report deals principally with the choice of question words in interrogative utterances for particular functions rather than with structural aspects of the entire utterance. In a report in preparation the development of structure within the utterance will be considered.

Subjects and procedures

The forms which were traced in this analysis correspond largely to English wh-question words. The French forms will be referred to as Q-words where the Q stands for "question" but happens also to be the first letter of a number of the Q-words, e.g., qui, qu'est-ce que, quand.

Kenny and Greg, the children whose acquisition of French L2 is discussed here, are Canadian children whose L1 was English and for whom French was truly a second language. That is, they had little exposure to French before they entered a pre-school program when they were about four years old. In spite of parents' expect-

Financial support for this project was originally provided by a National Science Foundation Dissertation Research Grant. Subsequent funding was provided under a grant to Alison d'Anglejan and G. Richard Tucker from the Québec Ministry of Education.

tations that this pre-school class would be conducted in French, English was frequently used and neither child produced more than a few French words and phrases at the end of the year. The following year, they entered a "French immersion" kindergarten. That is, all the children in the class were Anglophone but the teacher spoke only in French to them (see Lambert and Tucker, 1972; Swain and Bruck, 1976). In November of that year (1975) Kenny and Greg were transferred to a French class where they were the only Anglophones.

Data analyzed in this study were collected after the children had been in the French class 2½ hours a day for two months. After the summer holiday each child was enrolled in a regular French grade one class. The last language sample analyzed here was recorded in April 1977, thus after approximately 15 months of fairly intensive exposure to French. Both children are continuing their education in French.

Language samples were recorded approximately monthly in informal play with an adult native speaker of French.* In most of the sessions the two boys played together with the researcher, but in some sessions each child played individually with her. All sessions took place in the home of one or the other of the children. The same toys were used in each session in order to provide continuity and to provide a consistent context in which linguistic development could be traced. That is, given recurring situations and similar activities, changes observed in language could more confidently be attributed to language development rather than to response to changes in the non-linguistic situation. During the 15-month period discussed in this report, eleven samples of Kenny's speech and eight samples of Greg's speech were recorded.

In terms of mean length of utterance (MLU) the children's speech over the period of the study ranged from what Brown (1973) defined for L1 acquisition as Stage II (MLU 2.5 - 3.0) to Stage III (MLU 3.25 - 3.75) with the last two samples from each child falling in or just beyond Stage IV (MLU 3.75 - 4.25). These MLU values are undoubtedly slightly inflated because of the existence of a number of partially analyzed routines in the children's speech and by the computation method which biased the sample in favor of multi-word utterances by eliminating "oui", "non", and numbers chanted in a series (see Lightbown 1977a for a discussion of problems with MLU as an index for child L2 development).

* The research assistant throughout this project was Louise Tétreault. She interacted with the children in these play sessions and became their welcome guest. She transcribed all the tapes and helped in the processing of data.

Procedures for data analysis

All utterances which included Q-words were identified in each sample. Both single-word and multi-word utterances were counted. The proportion of the total utterances which included Q-words was computed for each sample. A second formal analysis was done to determine the proportional frequency of each Q-word within the total of Q-word utterances. Then the function of each of these utterances was examined. Further, the transcripts were examined to determine whether functions normally served by certain Q-words were served by forms which were not Q-words. Function was determined according to information about the linguistic and non-linguistic contexts of utterances. Results of the formal analyses are presented under Results below. The outcome of the functional analysis is presented in the Discussion section.

In addition, some other formal and functional analyses were performed on both the children's speech and the speech of their adult interlocutor. Findings from these latter analyses will be presented only as they contribute to explanation of the results of the major analyses.

Results

The proportional frequency of utterances with Q-words was similar for the two children and there was little change over time. In Greg's case, the consistency was particularly striking. At a minimum, .05 of the utterances in any sample included Q-words and the highest proportion was .20. The average proportional frequency was .12 for Kenny and .10 for Greg. The total number of Q-word utterances was 338 for Kenny and 335 for Greg.

The most frequent form in almost every sample - both numerically and proportionally - was "où". The next most frequent over all was "qu'est-ce que". For Kenny there was only one sample in which "où est ____" and "qu'est-ce que c'est" represented less than .50 of all the Q-word utterances. For Greg, the proportional frequency of these two forms was almost as great when the average frequency for the entire corpus is considered. However, in some samples there were no occurrences of "qu'est-ce que c'est". In the first sample of Greg's speech, "qui" was the most frequent Q-word. As will be seen in the discussion, "qui" had a rather unique role in Greg's development of questions.

Among the less frequent Q-words, "quoi" increased sharply over time and became the most frequent form in the last samples. "Qui" varied in frequency over time for both children and also in function for Greg. "Pourquoi" was not used more than twice in any sample except for the last three samples of Greg's speech. "Quand" appeared only twice in the entire corpus, both in the last sample of Greg's speech. Kenny used "comment" in two of the last samples of his speech.

Discussion

The observed sequence of emergence and relative frequency of Q-words (i.e., "what" and "where" earliest and most frequent; "how", "why", and "when" later and less frequent) corresponds in general to the sequence observed in L1 studies (Davis, 1932; Smith, 1933). In the speech of L1 learners the order of emergence of question forms is usually attributed to the cognitive development of the child. The first, and most frequent questions "where's x" and "what's that" are linked to the development of object permanence and to the related awareness that all objects have a name. The concepts of time, causality, and manner are acquired later and thus one would expect the corresponding question forms to be acquired later.

Felix has suggested that "at any given time the number of primary interrogative pronouns known to the L1 learner corresponds exactly to that of the corresponding cognitive concepts" (1976, p. 63). However, it is not the case that children must fully understand the function of these words or the concepts they encode before they use them. Certainly the terms are sometimes used as trials as children attempt to understand, for example, exactly what this "why" they hear so often really means. Meanwhile, concepts of causality continue to develop alongside the linguistic "why-because" exchange until eventually the relationships between causality in physical and psychological reality and their linguistic expression are better understood (see Blank, 1975; Hood, 1977; Piaget, 1923).

However, as reports of longitudinal studies of L2 acquisition emerge, a very similar order is apparent (see e.g., Felix, 1976). One cannot attribute this order to the lack of cognitive development on the part of these children nor to the absence of "why" or "when" forms or functions in their L1. The problem for L2 learners must be one of linguistic coding -- the expression of ideas. Thus the analysis of question function was undertaken to determine whether, at a given point in L2 development, the Q-word forms which Kenny and Greg used represented the range of question functions they encoded.

The functional analysis produced considerable evidence that the children developed several strategies which enabled them to ask questions in addition to those for which they had acquired the specific Q-word. Three of these strategies are described below: (1) the substitution of one Q-word for another; (2) the incorporation of whole interrogative utterances in larger units; (3) the use of circumlocution when a particular Q-word was not known.

Substitution of Q-words. Even though the "qu'est-ce que c'est" form did not appear in the first sample of Greg's speech, its function was served in utterances with the Q-word "qui". Most of the Q-word utterances in this sample had the form "qui est ça?" or "qui ça?" While playing with toy animals, Greg repeatedly asked Louise this question. Each time, the answer he accepted was, for example, "un crocodile" or "une chèvre". The context of these

questions was always play with the toy animals and it seems unlikely that Greg was really asking "who?" rather than "what is the name of?"

In the next sample "qu'est-ce que c'est?" had taken over this "name of" function, but "qui" had acquired another function - also different from its functions in the adult language. This new function could be translated as "what did you say?" or "please repeat". For example,*

(Greg pushes toy car under bridge)

L: c'est une autoroute en-dessous?

qui?

L: c'est une route en-dessous?

c'est un chemin en-dessous du pont?

mmhm. oui.

In the next sample of Greg's speech, "qui" still had the "what did you say?" function, but it was also used to mean "who" in full verb but not copular sentences, e.g., "qui a fait ça?" or "qui met-tre ça là?"**

In the next sample there were no occurrences of "qui" with verbs. It continued to be used in a single word utterance to mean "what did you say?" and began to appear as a pro-form for the noun in an Article / Noun utterance as a request for repetition or clarification of a previous utterance. For example,

(G playing with donkey)

L: c'est quoi ça?

ça c'est un bébé cheval.

L: un âne?

un ____.

L: un âne?

un qui?

oui. ça c'est un âne.

* In this example and those that follow, the child's speech appears on the right and the adult's speech appears on the left along with situational information which is in parenthesis.

** It has been noted elsewhere (Lightbown, 1977a, b) that in this sample "mettre" served as a pro-verb in cases where Greg did not know a specific verb. This use of pro-verbs was part of the general pattern of overuse of pro-forms which permitted these L2 learners to express a wider range of ideas than would be possible with standard use of the words and structures at their disposal. It was not always strictly a matter of lack of vocabulary but rather an apparent inability to use certain words in sentences.

In the next sample there were 83 Q-word utterances -- the largest number of questions in any sample. However, for the first time, there were no occurrences of "qui". The "what's the name of that?" function had been taken over by "qu'est-ce que c'est?" and "c'est quoi ça?", and the "what did you say?" function was served by "quoi?" which became productive for the first time in this sample. "Qui" was also absent from the next sample. In the last two samples of Greg's speech, all occurrences of "qui" functioned as requests for the name of a person.

Kenny also substituted one Q-word for another. As noted above, there were no examples of "quand" in the entire corpus of Kenny's speech. Indeed, even given the opportunity to imitate "quand" he chose an older Q-word as a substitute:

(G and K discussing their approaching birthdays)

L: vous voulez pas savoir c'est
quand ma fête à moi?

où est ton fête?

Before Kenny began to use "comment" he invented several ways of asking "how do you say that in French?" over time. An early version was probably "what's that in French?": "qu'est-ce que ça dans français?" but a later version appeared to be an attempt to ask "how" in the absence of the appropriate Q-word: "qu'est-ce que dire ça dans français?", (= how [do you] say that in French). The last recorded version was more target-like but still wide of the mark: "Comment tu es dire ça?"

The phenomenon of Q-word substitution has also been observed in other L2 studies. The most extensive report of Q-word substitution is in the study of German L2 by Felix (1976). He reported the case of a child who substituted wo (where) for both wann (when) and warum (why). Felix suggested that this phenomenon occurs only in L2 acquisition and not in L1 acquisition. In L1 studies of response to questions, however, confusion of Q-words has been reported. Ervin-Tripp (1970) noted that English L1 children sometimes answer questions containing unfamiliar wh-words as if they contained familiar words. For example, one child at age 2 years 7 months "gave almost all location answers for what-doing questions" (p. 84).

Adult: What's Papa doing?

He's there.

Some older children (3 years 1 month and older) gave because answers to when and how questions for a while. Tyack and Ingram (1976) found this kind of Q-word confusion to be fairly widespread. This evidence from children's response to questions in their L1 might appear to challenge Felix's suggestion (which he bases partly on Slobin, 1973) that Q-word confusion does not occur for L1. If, in

response to questions the child recognizes certain linguistic signals of question and answer, but without knowledge of the concepts they encode, he might be expected to make errors in production as well.

A search of the literature has revealed no evidence that, in production, L1 speakers substitute one Q-word form for another as do L2 speakers in this study and that of Felix (1976). However, some anecdotal evidence suggests that some children may (see e.g., the example from Clark (1974) in the next section). In any case, it seems clear that L1 children ask questions partly in order to learn what the questions mean by reflecting upon the answers they get (for example, see the discussion of "why" in Blank (1975)). L2 children, on the other hand, know the questions they want to ask, but they do not yet know the linguistic forms to encode them.

Incorporation of interrogative utterances. Sometimes it was not merely the Q-word which occurred where it would not have in the normal target-language, but an entire interrogative utterance. For example, the form "où est ___?" was not always used as a question about the static location of an object. Greg also used it, in the early samples, as an all-purpose locative question which could mean, for example, "where does this go?" A later version of the same question was "où est ça va?" Other examples include "qu'est-ce que c'est je fais avec ça?" These incorporated interrogative utterances appeared to be unanalyzed or only partially analyzed routines. That is, they were expressions which had been learned in certain contexts as whole units. When the utterances were used in new situations, the children gave evidence that they had not yet broken them down into their component parts (see Brown, 1968; Hakuta, 1975).

Wagner-Gough (1975) presented evidence that during his first month of exposure to English, her subject Homer (5 years old) used the wh-form "What is ___" in utterances which had no interrogative intent at all. Indeed, he answered questions posed by others by juxtaposing the wh-form of the question with his answer, e.g., Adult: "What is this?", Homer: "What is this truck?". Kenny and Greg did not extend their Q-word utterances this far. They seem to have recognized certain words as markers of the interrogative and to extend them within those limitations. They had either moved beyond a very early stage similar to that experienced by Homer or else there had been no such overgeneralization in their L2 learning.

The incorporation of whole sentences or phrases into longer utterances has been observed in L1 development (Clark, 1974). Clark argued that much of a child's early speech is made up of unanalyzed chunks which he fits into and combines with other structures in ways an adult would not. She recorded the following utterance from her son at age approximately 2 years, 10 months: "Where is our van over there?" She considered and rejected the hypothesis that it might mean "why is our van over there?" because no other causal questions had occurred in his speech. She concluded rather that the utterance resembled the incorporated interrogative utterances observed in L2 speech and meant something like "Is that our van over there?" with "Where is" used as a general interrogative

marker. Brown (1968) considered it likely that early wh-questions in L1 speech were largely unanalyzed routines whose component parts had not been recognized.

Other observers of L1 have found few examples of incorporation, noting instead a gradual expansion of utterance length by the realization of additional surface structure components - both content words and function words or morphemes (e.g., Bloom, 1970; Bowerman, 1973). Undoubtedly, there is variability in the extent to which L1 children use the incorporation strategy and there may also be differences in this strategy among L2 learners.

Circumlocution. "Quand" occurred only twice in the entire corpus - in the last sample of Greg's speech. However, the "when?" function was served by other forms as shown in the example below. As Louise was preparing to leave, Kenny wanted to determine when she would be coming back.

L: Quoi? Une autre fois?

a viens un autre fois?/

Oui/ jeudi/ vendredi/

Dimanche tu vas être ici?

There were many temporal expressions in the children's non-question utterances: days of the week, number of days, hours, or years before some anticipated event, "maintenant", "aujourd'hui", "tous les jours", etc. But the only use of "quand" as a question was in the last sample of Greg's speech.

Kenny neither used nor responded to questions with "quand". Very few questions with "quand" were asked of him and when they were asked Kenny did not answer. He answered other forms of temporal questions however, e.g., "En quelle année?" was answered "1977".

Summary

The development of Q-word forms and question functions in French L2 was traced for two 6-7 year old boys. The sequence of emergence of question forms was found to parallel closely that observed in L1 speech. However, these interrogative forms were overextended so that a wider range of question functions could be encoded. Furthermore, functions normally served by Q-words in adult speech were served by other forms in the speech of the L2 learners prior to their acquiring the appropriate Q-word forms.

REFERENCES

- Blank, M. Mastering the intangible through language. In D. Aronson and R. Rieber (eds.). Developmental psycholinguistics and communication disorders. Annals of the New York Academy of Sciences, 1975.
- Bloom, L. Language development: form and function in emerging grammars. Cambridge, Mass: MIT Press, 1970.
- Bowerman, M. Early syntactic development: a cross-linguistic study with special reference to Finnish. Cambridge University Press, 1973.
- Brown, R. A first language. Cambridge, Mass.: Harvard University Press, 1973.
- Brown, R. The development of Wh questions in child speech. Journal of Verbal Learning and Verbal Behavior, 1968, 7, 279-290.
- Clark, R. Performing without competence. Journal of Child Language, 1974, 1, 1-10.
- Davis, E. The form and function of children's questions. Child Development, 1932, 3, 57-74.
- Ervin-Tripp, S. Discourse agreement: How children answer questions. In J. R. Hayes, (ed.), New directions in the study of language. New York: Wiley, 1970.
- Ervin-Tripp, S. and Miller, W. Early discourse: some questions about questions. Pre-publication manuscript, 1976.
- Felix, S. Some differences between first and second language acquisition. Paper presented at the 3rd International Symposium on Child Language, London, 1975.
- Felix, S. Wh-pronouns in first and second language acquisition. Linguistische Berichte, 1976, 44, 52-64.
- Hakuta, K. Prefabricated patterns and the emergence of structure in second language acquisition. Language Learning, 1974, 24, 287-298.
- Hood, L. A longitudinal study of the development of the expression of causal relations in complex sentences. Unpublished PhD dissertation, Columbia University, 1977.
- Lambert, W. E. and Tucker, G. R. Bilingual education of children. Rowley, Mass.: Newbury House Publishers, 1972.

- Lightbown, P. M. Consistency and variation in the acquisition of French. Unpublished PhD dissertation, Columbia University, 1977a.
- Lightbown, P. M. French L2 learners: What they're talking about. To appear in The Proceedings of the First Annual Second Language Research Forum, University of California at Los Angeles, February 11-13, 1977b.
- Piaget, J. Le langage et la pensée chez l'enfant. Neuchâtel: Delachaux & Niestlé, 1923.
- Slobin, D. I. Cognitive pre-requisites for the development of grammar. In C. A. Ferguson & D. I. Slobin (eds.). Studies of child language development. New York: Holt, Rinehart and Winston, 1973.
- Smith, M. The influence of age, sex, and situation on the frequency, form and function of questions asked by preschool children. Child Development, 1933, 4, 201-213.
- Swain, M. and Bruck, M. (eds.). Immersion education for the majority child. The Canadian Modern Language Review, 1976, 32.
- Tyack, D. and Ingram, D. Children's production and comprehension of questions. Journal of Child Language, 1977, 4, 211-224.
- Wagner-Gough, J. Comparative studies in second language learning. CAL ERIC/CLL Series on Languages and Linguistics Number 26, June 1975.

LANGUAGE AND PERCEPTION

Donald Preziosi
MIT

In his 1976 address to the Third Lacus Forum, Dwight Bolinger observed that

The meanings of sentences are not just the expression of reality; they reach out to reality to discover whether they have a place in it, even whether they can create it (Bolinger, 1977;=2).

Language, he says, "...has evolved from our need to cope with reality."

In the present paper I would like to address the question of the role of language as an instrument of perception, and to consider the thesis that language has evolved not only as an instrument of interpersonal communication, but also, and possibly even more fundamentally, in order to provide speakers with the means to create a knowable world.

I shall consider this question both phylogenetically and ontogenetically - that is, from the perspectives of human evolution and individual development. Furthermore, I would like to consider the growth of language as the growth of one such instrumentality among a network of others - the others being, most importantly, the system of visual and spatial signs comprising what I have called elsewhere the built environment (15, 16).

This is by no means a new thesis, and it has been long recognized that language serves a variety of functions (7, 17); but a consideration of the perceptual functions of language is today receiving closer attention by linguists, cognitive psychologists, and semioticians (9, 14, 17), and it may be useful to note a convergence of suggestions drawn from a variety of different disciplines.

Our earliest ancestors, it is widely assumed (9), were non-tree-living primates who for one reason or another had entered an ecological niche primarily dominated by predatory carnivorous mammals. It has been suggested that the evolution of the human line was a response to adaptation for life in such a niche.

The geographical range of these hominids would have been considerably more extensive than that of other living primates,

and they would have had to cope with life in such an extended food-gathering territory with few of the traditional sensory advantages of their carnivorous competitors. Social mammals such as wolves, for example, employ olfactory labelling to mark their environments and thereby map a perceptual world; such a device would not have been available to the early hominids, with their much-reduced sense of smell (characteristic of the primates in general in comparison with other mammals).

I would like to suggest that the evolution of language came about as one of a series of powerful solutions to the problem of creating a real world which provided an adequate model for the sensory events encountered by individuals. Animals in general, and particularly so the primates, employ any available portions of their bodies to measure variation in the environment. In contrast to what was available to other mammals, the development of adequate labels to mark and distinguish an environment required, in the hominids, the development of more powerful auditory, visual and vocal capacities.

It would be adaptive if information received from different sense modalities could be given a common code or label, yielding a perceptual world in which sensory information was encoded as information about objects or events in space and time. Such an integrating code would work by marking or labelling stimuli in the different sense modalities - sight, smell, hearing and touch - as coming from the same object in space at a particular time. The conscious experience is essentially a construction of nervous systems for the handling of incoming information in simple and consistent ways - a model, so to speak, of a possible world, which enables the brain to handle the enormous amount of information at its disposal (9).

It is the thesis of this paper that language serves as an auxiliary sense-modality, a further refinement and integration of biologically-given perceptual apparatus. This is not a new thesis (3) but is one which is receiving increasing attention by writers today in a number of different fields, and, it would seem, increasing support. It may be suggested that such a thesis accords well with a consideration of the peculiar features of human language in contrast with the sign systems of other species, and with the peculiarities especially of primate communication as opposed to communication in non-primates. It also accords well with recently-elucidated design properties of other human sign systems (15, 16).

If we look at primate behavior, we may note a curious fact: sign systems, whether visual or vocal, reveal a strangely blurred identity with respect to the nature of their component entities. Whereas a non-primate such as a bird or fish will respond to a single patch of color or an individual ges-

ture - and often little else - the primate more consistently tends to act on the appearance of an entire body in space and time: its posture and vocalizations, as well as the history of previous encounters with another individual or group. Primates reveal a tendency to respond to information from more than one sense modality at a time, and manifest a simultaneous summation of complex sets of signals in interactive behavior. Among humans and other primates, individual relationships are personalized, finely graded, and rapidly changing, and there tends to be a premium on the precise expression, representation, and communication of mood (10, 18).

The primate lives in a complex social field in which it responds to many individuals simultaneously, and is often involved in behavior which is a compromise among a set of possible actions, each of which is subtly weighed against the other within a social context. Individual social fields are conceptualized in terms of various types of "attention structures" (4) - that is to say, it is a crucial social fact for an individual to know whom to pay attention to in what way at a given time. Primates - both human and non-human - are continually assessing the flux of behavior of their fellows in order to establish bases for their own behavior. Attention may be unilinear and directed, as in the case of dominance hierarchies in chimpanzees or human corporations, or reciprocal, as in the case of human and non-human mother-child dyads.

The ability to predict behaviors is crucial to the evolution of complex social systems. Among the social insects, differences in social, economic and reproductive activities are distinctly signalled by gross differences in somatic form (19); among the order of primates, there is an increasing tendency to minimize somatic differences in role behavior, a tendency reaching its apex in the human line, where apart from differences in reproductive roles, any individual can potentially replace any other in a social system. It seems reasonable to suggest that such minimalization has been a concomitant of the increasing reliance upon complex sign systems to signal social states.

Primates communicate their motivational states by means of complex displays which can include both physical posturing and movement, and vocalizations. Human and non-human primates are adapted to the perception of such gestalts which incorporate various types and grades of information simultaneously. Among humans, such perception has been refined to an extraordinary degree by the development of the cultural way of life wherein nearly anything perceptually palpable can be potentially employed to calibrate and measure environmental and social variation over space and time.

Not only do human sign systems differ fundamentally from those of our closest living primate relatives, the chimpanzees, in being designed according to a principle of double articulation or duality of patterning (1, 6, 7), human sign systems utilize "form" not as a fundamental constancy but rather as sets of parameters within which variation may take place. And while human sign systems, like those of other advanced primates, manifest a tendency toward composite formation wherein visual and auditory signals can be blended with tactile clues to form complex signals, in the human line there is manifest a high degree of generalization, flexibility and redundancy such that each sign system can be employed as a substitute for others in given contexts.

In other words, humans switch codes and mix metaphors continually; rather than being a perversity, this characteristic is one of the cornerstones of all human semiotic behavior. For some reason, it has been crucial to our survival as a species not only to employ given sign systems, but to dispense with one or another when it has been useful to do so.

Each human sign system in the ensemble of sign systems which comprise human culture has been designed so as to be semi-autonomous of other systems, or partially-redundant with respect to other systems. Each system can be employed to encode information coming from a variety of sense-modalities. A linguistic text can partially-encode the visual information of a painting or a building, just as a painting may partially-encode verbal information. Such encoding is a partial or perspectival view induced by the geometries or logic of a given system. Seen from the framework of language, a built environment may appear text-like (11); but it is not so in se - rather it may be useful to look at it in a certain way for specific purposes. One of the chief pitfalls of contemporary semiotic study of the visual and architectonic systems has been to mistake the color of the landscape for that induced by the spectacles of the viewer (16).

It would appear to have been adaptively important in the early history of the human line for sign systems to have been designed in this flexible, mutable and partially-redundant way. It is not clear why this has been so, and the subject has been addressed on many occasions (see 10, 5, 14, 21). At any rate it is clear that human language is a sign system built upon entities (such as phonemes) which are less like the bricks of a building and more like conventionally-defined ranges of variation within which bricks might be made. It is also clear, as our own work at MIT has now demonstrated (16), that certain non-verbal sign systems are similarly organized.

I am suggesting that it may be useful in building up a reasonable picture of the evolution of human language not to ignore the role of language as an instrumentality of perception, for I think that a number of the peculiar properties of the organization of language systems may be more adequately addressed. In addition, as I will note below, certain aspects of child language-learning may be similarly clarified. By not forgetting the role of language in perception we may develop a picture of human language evolution more inherently plausible than that offered by a model which sees language as merely an over-complex primate call system (see 12).

While some recent writers have suggested that the role of human language in interpersonal communication first evolved as a side-effect of its role in intrapersonal reality-construction (9), I think that such a view is overly constricted; human language is inherently multi-functional (in contrast to nearly all other animal communication; but see 17), and I do not believe that its peculiar organization (vs. non-human sign systems), its flexibility and creativity, could be otherwise accounted for. It has been suggested (9) that if there had been selective pressures toward the development of language specifically for interpersonal communication, then we would expect the evolutionary response to have been in the direction of "prewired" language systems with invariant conventional symbols. The latter appears to be the typical approach to interpersonal communication in other vertebrates, and it is accomplished (as, for example, in birds) with little or no learning and with remarkably small brains.

I do not believe that human language arose outside of the general social milieu shared by other primates - in which sign systems are inevitably complex and multi-functional - and while the argument that human language arose largely as a perceptual instrument may have cogency, I do not think that this was its only function, even from the outset. To claim that the role of language as a perceptual device is phylogenetically prior is to ignore its complex primate background. (16).

There is also a good deal of confusion regarding what is and what is not "prewired" in human communicational systems. The ongoing study of linguistic universals (5, 8) suggests that a number of design features are shared by all human languages. It appears to be the case that not only are speech-sounds processed differently by the brain from environmental sounds, there now appears to be convincing evidence for the innateness of aspects of the distinctive feature system itself upon which all human languages are scaffolded (8).

If we look at the situation of the perceptual function of language from the ontogenetic side, there would now appear to be interesting evidence for the early development of this kind of language use.

Babies are not born speaking the language of their parents but do appear to be born with an innate capacity for learning that language. I would suggest that this capacity itself is a concomitant of an innate ability of human infants to find out about their environment (see 14). Practically from birth, infants are tracking their environments with their eyes and ears, before they have learned to focus their eyes or distinguish one kind of sound from another.

Since speech is something perceivable, it may be useful to consider the kinds of information it offers the prepared listener. Sounds inform us about events. While vision and touch enable us to explore static and mobile objects, hearing tells us principally about objects in motion. Just as an optical array is specific to objects that reflect light, so the acoustic patterns that reach our ears are specific to the identity, location, motion and meaning of the sounding objects (14).

To speak is to make finely controlled movements in certain parts of the body, with the result that information about such movements is broadcast to the environment. In this sense the movements of speech can be considered articulatory gestures. When we perceive speech, we are picking up information about real, tangible events occurring in some other person's mouth. But speech sounds are not random environmental events, and we process them differently in our brains. Furthermore, we perceive such sounds as meaningful: prepared listeners attend to the meanings of speech sounds, and such meaningful patternings give coherence to such acoustic continua. (It may be that there are certain linguistic parallels to the principle of economy in visual perception wherein it is apparently easier to order complex two-dimensional patterns as, for example, a representation of a cube; see 16, Section 6).

It appears to be the case that speech perception begins as one aspect of the general perception of other people's movements, to become more sharply differentiated over time. Since speech perception has a number of things in common with the perception of other bodily motions, the perception of facial expressions and body language should in some sense be continuous and partly-redundant with it. Children seem to be as interested in making sounds as in looking at, touching, smelling and manipulating objects. And in an interesting way, these activities have something in common: the perceiver is engaging in certain muscular movements and adjustments which result in making certain kinds of information available. In seeing, the

movements of the head, eyes (and often the body) produce or make available new optical information; in vocalizing, these movements generate new auditory information.

A number of writers have suggested that there is an inseparable connection for an infant between vocal labels and the characteristic optical and tactile features of objects (13, 14, 21); indeed, the vocal label tends to be registered as a necessary characteristic property of the given object. In other words, the child tends to treat such signals as belonging in some way to the object of attention - to the same object in space and time. This is not unlike attributing voice sounds coming from loudspeakers at the side of a cinema as coming from the projected images on the screen itself. Auditory information tends to be susceptible to incorporation into cognitive anticipatory schemata of other modalities (14) with particular rapidity and ease.

This does not mean, of course, that the child cannot localize acoustic signals in the face of say, her parent when the latter pronounces "Rover" when the latter trots into the room, only that children and others tend to attach the names of things to the things themselves - in other words, that they tend to incorporate the names of objects into the anticipatory perceptual schemata by which the objects themselves are perceived. The name of something is easily incorporated as one of a series of properties of a thing. Interesting confirmation of this situation is offered in recent work on spatiokinetic behavior (20), suggesting that children do not perceive meaning as being contained solely in words but redundantly in accompanying gestures (or only partly-redundantly). Other primates apparently behave in the same way (10).

I would concur with the view that making vocal sounds is in certain respects analogous to perceiving. Both activities involve doing things to make information available. Because the two schemata are already linked, saying the name of something may seem to a young child much like other exploratory movements she makes to bring new properties of an object into view. Such behavior is certainly consistent with the use of language for invocational purposes in ritual, religion and myth (11).

It would seem to be the case, then, that there is a stage in ontogenetic development when naming or labelling things vocally is deeply embedded in the very processes of perceiving them. For the child, an argument can be made to the effect that the spoken word begins as an integral part of some larger perceptual activity, later becoming detached from that activity and enlisted in the service of other cognitive schemata.

Like the non-human primate, the young child is engaged in complex, multi-modal perceptual cycles incorporating information in mental schemata derived from a variety of sources, arrayed together in a kind of "rebus" (16) characterized by an increasing tendency toward detachment and decontextualization of perceptual information (here the primate analogy stops). The growth of the child from infancy to maturity, as Piaget and others have observed (23), involves not only an increase in complexity and sophistication of cognitive schemata, but at the same time a disengagement of individual systemically-organized ways of ordering the world. Each of these cognitive modalities offers a framework on every other modality, and on the totality of informational resources comprising a culture.

Every human sign system offers a focussed, limited, and partially-redundant map of the perceptual world. Such systems are at the service of human users who may employ them for a variety of ends, substituting now one or another, or combining several. The extraordinary richness of human culture is at least in part a result of this complex, multidimensionally-integrated way of perceiving.

It is a long swim from ontogeny to phylogeny and back again; nevertheless one can perceive a convergence of equivalent positions on the role of language as a modality of perception. Such a convergence belongs to the future, and is itself subject to the threads of potentially converging argumentation in cognate disciplines, particularly in the area of the study of sign systems in general. If a case for the phylogenetic primacy of the role of language as an instrumentality of perception cannot be cogently made (as I suggest here), a focus on the subject from an ontogenetic perspective may enlighten our picture of the development of language-learning in the child, since there appears to be increasing evidence of the importance of this function in early learning. It might well turn out to be the case that the various functions of language are learned in tandem, in an equilibrated and overlapping fashion not unlike the sequencing of the learning of various kinds of spatial concepts in visual perception. In the case of the latter, mature perceivers manifest an equilibrated series of different kinds of spatial models of the world (topological, projective, geometric) which have been shown (see 23) to have been learned in sequence through childhood; it may well be that the various functional roles of language are similarly learned in tandem, yielding, for the adult speaker, the complex multifunctional system of human language. (24)

REFERENCES

- (1) E. Benveniste, Problems in General Linguistics (Miami, 1971)
- (2) D. Bolinger, "The Price of Language," The Third Lacus Forum (Columbia, S. Carolina, 1977) 3-11.
- (3) J. Bruner, On Knowing (Cambridge, MA, 1962)
- (4) M. R. A. Chance & C. J. Jolly, Social Groups of Monkeys, Apes & Men (New York, 1970)
- (5) J. Gressberg, ed., Universals of Language (Cambridge, MA, 1966)²
- (6) C. F. Hockett & R. Ascher, "The Human Revolution," Current Anthropology, 1964.
- (7) R. Jakobson, Main Trends in the Science of Language (New York, 1973)
- (8) R. Jakobson & L. R. Waugh, The Sound Shape of Language (ms.; forthcoming, 1978, Bloomington, Ind.)
- (9) H. J. Jerison, "Palaeoneurology and the Evolution of Mind," Scientific American, January 1976, 90-101.
- (10) J. B. Lancaster, Primate Behavior and the Emergence of Human Culture (New York, 1975)
- (11) E. Leach, Culture and Communication (New York, 1975)
- (12) E. H. Lenneberg, "Language, Evolution and Purposive Behavior," Culture in History, New York, 1967.
- (13) A. R. Luria, "Scientific Perspectives and Philosophical Dead-ends in Modern Linguistics," Cognition 3 (1975) 377ff.
- (14) U. Neisser, Cognition and Reality (San Francisco, 1975), esp. 154-176.
- (15) D. Preziosi, "Toward a Relational Theory of Culture," The Third Lacus Forum (Columbia, S. Carolina, 1977) 278-288.
- (16) _____, Architecture & Cognition: An Introduction to the Study of the Built Environment (forthcoming), esp. part 6.
- (17) T. A. Sebeok, Perspectives in Zoosemiotics (The Hague, 1972)
- (18) J. van Lawick-Goodall, In the Shadow of Man (Boston, 1971)
- (19) K. von Frisch, Animal Architecture (New York, 1974)

- (20) W. von Raffler Engel, "On the Early Acquisition of Kinetics," The Second Lacus Forum (Columbia, S.Carolina, 1976)
- (21) L.S.Vygotsky, Thought and Language (Cambridge, MA, 1967)
- (22) G.A.Miller & P.N.Johnson-Laird, Language and Perception (Cambridge, MA, 1976)
- (23) J.Piaget, The Origins of Intelligence in Children (New York, 1963); id., Genetic Epistemology (New York, 1970); id., Biology and Knowledge (Chicago, 1971).
- (24) See now in this regard R.Jakobson, Die Grammatische Aufbau der Kindersprache (Berlin, 1977)

MATERNAL DIGLOSSIA:
THE ADULT USE OF "BABY TALK" AS AN INSTRUCTIONAL DEVICE

Valerie Becker Makkai

University of Illinois at Chicago Circle

It must be stated at the outset that this paper does not report on any sort of controlled scientific experiment regarding the adult use of baby talk and its effects on the child. Rather it offers the author's personal observations of the phenomena to be discussed. The possibility of making controlled studies on the subject will be discussed below.

It is a widespread phenomenon in language that adults, especially women, and particularly the mother, when talking to infants and young children, often indulge in some form of "baby talk". Such baby talk falls into various categories. One of these types is the deliberate mispronouncing of words which the adult feels would be difficult for the child---for example dada for daddy, titty-tat for kitty-cat, goggy for doggy, etc. Often these mispronunciations are done in imitation of a pronunciation that the child himself has given the word. Another type of "mispronunciation" involves the distortion of the total utterance through such things as consonant substitution, changing of voice quality or intonation patterns, and possibly lip protrusion or rounding, for example, Ooh, what a pwetty baby---idn't tye [she] a tweety-pie!

Another form of baby talk involves use of a special "easier" vocabulary such as bow-wow for dog, choo-choo for train, or baby to refer to any young child, whether really a baby or not, or birdie to refer not only to birds but also to ducks, swans, pelicans, etc.¹ Again some of this may be done in imitation, or supposed imitation of what the child himself might say.

Now the usefulness of all these forms of baby talk is highly doubtful. It is not at all clear that the adult is helping the child in any way in his effort to acquire the sounds and vocabulary of his language. Neither, however, is it clear that such baby talk does any harm by inhibiting, in some way, the child's development. These things remain to be investigated more thoroughly before any conclusion can be reached.

However, there is one form of baby talk which appears to be clearly useful to the child, and that is the device of referring to all individuals, including oneself, in the third person. For example, a mother may say to her baby such things as:

Is baby hungry? or Is Sally hungry?
Mommy's getting a nice supper for baby.
She just has to warm up the bottle first.

¹Paul Garvin has made the interesting observation that ducks, swans, and pelicans are all birds but they are not properly birdies.

It is interesting to note that this kind of device seems to be universal. I have yet to find a language which does not have something very similar. Of course, languages which use verbal affix markers of person in addition to, or instead of, pronominal markers undergo the same kind of shift to third person, which, then, is reflected in the verb as well. It appears that adults the world over must have an unconscious awareness of the difficulties that arise in learning the personal pronoun systems of their languages. And these are, indeed, significant difficulties, since the referent of a pronoun is constantly shifting. With a noun, the referent or referents are fairly stable. For example, a child may learn very easily what the sun is because there is only one of them and it always looks approximately the same---even if it does move through the sky and appear sometimes to be different colors. The word tree should present no great difficulties, because, even though the child may see trees of different sizes, shapes, and colors, they will always be called trees (at least by adults), and not cats or spoons or chairs. The child may have difficulty learning what the semantic range of an item is---for example, he may try a few times to call a bush or even a flower by the word tree until he learns better---but the uses of the word tree itself by the adults around him should remain consistent and unambiguous. Thus the child is confronted by a relatively stable model from which to learn. But consider the confusion that pronouns must create in a small child's head. One and the same person may at different times be referred to as I, me, my, mine, myself, you, your, yours, yourself, she, her, hers, herself, Mary, mama and so on. The child himself may be called Tommy, baby, you, your, yours, yourself, he, him, his, himself and (eventually) I, me, my, mine, and myself. To look at it from the opposite side, the single word I can be used to refer to absolutely anyone in the child's experience, as can in fact each of the pronouns (with the obvious sex restrictions for he and she and their related forms). This situation frequently results in a complete confusion in the child's mind as to what pronoun to use when, and it is one of the standard features of early child language acquisition in most languages that the pronouns are confused in various ways.

The confusion may take various forms. Sometimes a child will refer to himself in the second person, calling himself you, so that he will say You want a glass of milk meaning I want a glass of milk. One very frequent occurrence is the child's use of me or my instead of I---for example Me want to sit in mommy's lap or My want to play with Billy's train. Here the child has chosen a first person form, but is confused as to which first person form to use. It is interesting to note that the use of my, and even mine, for I occurs especially frequently in children who have older siblings. Presumably they hear my and mine used very frequently by their siblings, as in That's mine or You can't play with my train, etc. Children often refer to themselves in the third person, calling themselves by name or using the word baby or some equivalent term, but surprisingly it is rare for a child to refer to himself with a third person pronoun, even when he has been exposed frequently

to the use of third person pronouns as a baby-talk device by the adults around him. Apparently the adult use of the third person does not confuse the child, but actually eases the child's transition into the adult language. If the mother consistently refers to herself as she, mama, etc., the baby is confronted with a much more stable relationship of word to referent. Theoretically it would be possible to use only third person forms and completely omit first and even second person forms when talking to the child. But in fact such a consistent pattern is rarely found.² The adult may frequently use third person to refer to himself but vascilate between third and second person to refer to the child. For example:

Is baby hungry? Don't cry---Mommy's getting
your bottle.

The above type of sentence is probably much more common than the following:

Is baby hungry? He shouldn't cry---Mommy's getting
his bottle.

Some vascillation between third and first person forms may also be found, but it is less frequent and usually does not occur within a single sentence. Thus one might find:

Mommy's washing her hands---then she'll get your bottle.

or possibly:

Mommy's washing her hands---then I'll get your bottle.

but not:

*Mommy's washing my hands---then I'll get your bottle.

With so much vascillation, one might think that adults would actually be contributing to, rather than alleviating, the child's confusion, but what evidence we have actually points to the contrary.

It is difficult, of course, to find examples of children who have not been exposed to this type of baby talk, since most adults indulge themselves in it at one time or another. However, we have one excellent study of a child who apparently had minimal exposure to baby talk, and that is Michael Halliday's study of Nigel, as reported in his book Learning How to Mean (Edward Arnold 1975). An examination of the dialogs reported in that volume will show that Nigel's parents almost never used the "third person" device in talking to Nigel. Whether or not this was a conscious, deliberate decision on their part does not matter here. What is of particular interest is the pronoun system which Nigel evolved for himself. It involved, in fact, a complete reversal of the proper role of the pronouns, so that he consistently referred to himself as you while he referred to those whom he was addressing either as I or else with a noun form such as Mummy or Dada. This was accompanied by other reversals as well, so that we find examples such as:

When music finish then I sing you duck song. meaning

²Interestingly, those few children who begin by referring to themselves as he or she seem to be ones whose parents have made a conscious effort never to use anything but third person forms with the child.

"When the music is finished you're going to sing me the duck song, aren't you?"
Have you made a church spire? meaning "Look, I've made a church spire!"
Did you drop the green pen? meaning "I dropped the green pen."
 Dada got scrambled egg...Mummy get foryou scrambled egg! meaning "Daddy got scrambled egg...now get some for me!"
 That white pin prick your [my] tummy...that white pin prick your [my] knee.
 Where the little chuffa [train]...you [I] want the little green chuffa that you [I] found on the pavement.

Now, of course, Nigel is only one child, and other children who have never been exposed to baby talk might react differently. But it is significant, I believe, that Nigel's is the most complete reversal of pronoun referents that is possible. It shows that the system he constructed for himself (which, of course, soon began to break down as he gradually acquired the adult system) was based on a complete misunderstanding of the meanings of the pronouns. He had in fact attached to the pronoun you the single and inalterable referent Nigel. That is, only Nigel could be called you---in this sense it was like another name for Nigel. Similarly I or me always referred to the person he was talking to, never to Nigel himself.

It must be remembered that the concept of the pronoun, as such, is no doubt a difficult one for a child to grasp. He is used to things having names, and since people are just more objects to be named, his first inclination, no doubt, is to regard the pronouns he hears in the same way as he would the names of objects that he encounters. In fact, as we all know, the basic difference between nouns and pronouns is that pronouns can substitute for nouns---that is, they can refer to any number of different objects each of which would be referred to by a different noun or name. For a child who is trying to learn the names of things, such a word as you or she will at first seem just like any other name. Only when he begins to realize that these names are different because they can be applied to a wide variety of objects, will he begin to be able to use the pronouns correctly.

When the child hears only third person forms to refer to all the people in his experience, including himself, he has fewer forms to have to sort out---he doesn't have to worry yet about the specific meanings of first and second person forms.³ Also, given

³Also he thereby avoids the complexities of varying person markers on verbs and of subject-verb agreement. Kenneth Pike points out that in training his students to do field work elicitation he instructs them to introduce first and second person forms as late as possible in the investigation. This not only gives a much simpler verb structure to work with, but also avoids the common problem in field work of misunderstandings between the informant and the linguist as to who is meant by "you" or "I".

only a few forms to deal with, he is probably better able to learn the pronominal sense of these forms, since he becomes aware much sooner of their widespread application. When first, second, and third person forms are all used with the child, it may be a while before it dawns on him that these are not just more names for the people in his experience. Part of Nigel's trouble may have been based on the fact that, since he never heard himself referred to as anything but Nigel or you, and these forms seemed not to be applied to anyone else, he took you to be just another name for himself. But a child who hears both herself and her mother referred to as she must begin to realize that she is not like other names.

Obviously a great deal more study will have to be done before we can show, definitively, that the use of the third person does indeed aid a child in learning the pronoun system of his language. However, the problem here is, how to make such studies. It might be nice if we could have two groups of children, one group containing children who are never exposed to this baby talk device, the other containing children who are spoken to only using this device. But members of the former group would be very hard to find. Furthermore, as was pointed out, adult use of baby talk with children is not always consistent, so it would be difficult to find children for the second group as well. And even if we could find parents who use baby talk consistently with their children, the situation still is complicated by the fact that the child will no doubt overhear conversations between adults, in which baby-talk, obviously, would not be used. In fact the large majority of children are usually spoken to in baby talk, but not always, and they frequently hear around them speech which does not involve baby talk. Thus they hear their mother refer to herself as mommy and she but also as I. And they hear themselves referred to as baby and he or she, but also as you. While this may appear to confuse the child, perhaps in the long run it doesn't. That is, perhaps it is useful, on the one hand, for the child to realize that the referents of a pronoun can occasionally shift, while on the other hand being presented at first with a situation where, for the most part, they do not shift.

Obviously, what has been stated above is merely a hypothesis. It is based on a great deal of personal observation, but it has not been proven experimentally. And our problem in testing such hypotheses is that we cannot construct for ourselves any sort of controlled experimental situation. As with all studies of child language, we have to merely observe what the children do at various stages of development and in various environments. For this reason I seriously question the validity of any study which includes only one or two or a handful of children. It may prove what those children do, but it does not really say anything about what other children may or may not do. It is my contention that to test any hypothesis about child language acquisition, including the present one, we need to study a large number of children in a large variety of linguistic environments. Only then can we hope to have averaged out all the external factors that may affect one or another individual child in his linguistic development.

II

PSYCHO- AND NEUROLINGUISTICS: LANGUAGE AND THE BRAIN

SLIPS OF THE MIND
Gary S. Dell
University of Toronto

Many people have the feeling that they can hear a little voice inside their head. They hear it when reading, when writing, when thinking. This is the phenomenon of inner speech. The Soviet psychologist A. N. Sokolov (1972) defines inner speech as "soundless mental speech, arising at the instant we think about something, plan or solve problems in our mind, recall books read or conversations heard, read or write silently."

Inner speech under various names has played a role in a variety of theoretical formulations, including theories of short-term memory, where it has been called the verbal or articulatory code; and reading, where it is known as phonemic recoding. But inner speech is probably most important theoretically because it illuminates the age-old problem of the relationship between language and thought. If inner speech tends to accompany thinking, then it might be argued that language plays a crucial role in human thought processes.

In order to demonstrate that thinking is dependent on language via inner speech, one must meet three very stringent assumptions:

- (1) Inner speech must be shown to accompany all types of thinking.
- (2) It must be shown that inner speech is not merely an echoing of thought processes, but rather a crucial component of the thought processes themselves.
- (3) Inner speech must be shown to be genuine speech, and not some quasi-linguistic phenomenon.

It is this final point, that of the speech-like nature of inner speech, that this paper will deal with.

Many people have argued that inner speech is quite different from overt speech. They stress its fragmentary nature, its lack of a syntax. Vygotskii (1962) claimed that inner speech has what he calls a "predicative" quality. He writes, "in inner speech it is never necessary for us to name that about which we

are speaking, i.e. the subject. We limit ourselves only to what is being said about this subject, i.e. to the predicate." Not only is the syntax of inner speech incomplete, but the phonology as well. Anan'ev (cited by Sokolov) argued that words in inner speech are only represented by their initial consonant sounds; the vowels and other consonants are not present at all.

In opposition to these views, Sokolov and others believe inner speech to be very speech-like. In a number of experiments Sokolov has found that inner speech is similar to overt speech in that it is characterized by small muscular movements in the articulatory organs. Members of the behaviorist school of psychology e.g., Watson (1919), saw inner speech as being equivalent in every respect to overt speech, except for the presence of sound.

Although the finding of articulatory movements in inner speech does lend support to the hypothesis that inner and overt speech are similar, it does not demonstrate their formal equivalence. Some kind of articulation may be involved in both inner and overt speech, but it is unclear whether the content of the articulation is the same.

I would like to introduce a phenomenon which clearly shows the equivalence of form between inner and overt speech - inner speech is characterized by slips of the tongue just as overt speech is, and moreover, these slips of the tongue in inner speech (hereafter called slips of the mind) follow the same laws as those in overt speech.

To support this claim, I carried out a small experiment where both slips of the tongue and slips of the mind were elicited from some tongue twisters. To create slips of tongue, subjects were asked to say the tongue twisters listed in Table 1 in time with a metronome so that the speaking rate was 2.4 stressed syllables per second. Each tongue twister was spoken four times and a tape recorder recorded all the slips that resulted.

TABLE I
Tongue twisters used in the experiment

Thirty-seven silver thistles
A stewed sow's snout
Rubber baby buggy bumpers
A bucket of blue bug's blood
I think I've seen a single thong
A proper copper coffee pot

Slips of the mind, however, are not so easy to observe and record. A second group of subjects was asked to imagine (instead of say) the tongue twisters in time with the metronome. These subjects were directed to not move their tongue or lips. As was the case with the first group, this second group of subjects did each tongue twister four times at a rate of 2.4 stressed syllables per second. Since one cannot hook up a tape recorder to the brain of the subject, subjects were asked to report any slips of the mind that they detected. For both groups of subjects, the test trials took place without the printed tongue twisters in front of them.

Before the data are introduced, I would like to discuss potential problems with this technique of eliciting slips of the mind. Because subjects are required to report on the contents of their consciousness, this technique is essentially one of introspection. Although introspection is often employed in perceptual experiments and seems to be a common methodology in linguistics, it is regarded with much suspicion by experimental psychologists. The main reason is that one never knows whether the subject is unbiasedly reporting conscious experience or whether his report is contaminated by his expectations as to what his experience should be. With regard to slips of the mind, it could be the case that the reported slips of the mind were not slips of inner speech, but instead were fabrications based on the subjects' intuitions about slips of the tongue.

There are three aspects of the data which point to the genuine nature of the obtained slips of the mind. First, before subjects were told about slips of the mind, they were asked to familiarize themselves with a practice tongue twister by imagining it to themselves. Two subjects spontaneously reported that they made a slip of the mind. Secondly, the subjects acted as if they were surprised and amused when they detected their first slip of the mind. Finally, as will be seen, some slips of the mind involved complex feature substitutions, which, although quite lawful in themselves, could not possibly have been fabricated by the subjects, none of whom were familiar with linguistics or the study of slips of the tongue. So it is likely that when subjects reported slips of the mind, they were reporting genuine experiences rather than expected errors.

Turning to the results of the experiment, the first point to note is that slips of the mind were surprisingly about as frequent as slips of the tongue. There were 26 slips of the mind versus 24 slips of the tongue. Many of the errors occurred both as slips of the tongue and slips of the mind. In Table 2 a sample of slips of the mind is given and they are labeled by category names that have often been used to describe slips of the tongue (Fromkin, 1971). There were errors of anticipation,

where a phoneme emerges before it should; perseveration, where a phoneme erroneously repeats itself; and transposition, where the order of two phonemes is reversed. Sometimes an incorrect phoneme appeared even though that phoneme was not present in the surrounding words, such as the slip a stewed sow's snout → a stewed sow's shout. Just as is true of overt slips, slips of the mind involve different sized linguistic units. The slip blood → bug can be analyzed as an error at the level of the word. In the slip buggy → bunky we have a complex feature anticipation error. It appears that the voicing and manner of articulation of the cluster /mp/ has been anticipated and blended with the back place of articulation from the /g/ in buggy to create bunky. Such complex feature errors are not uncommon in normal slips of the tongue.

TABLE 2
Sample slips of the mind

1. Anticipation of a phoneme - bucket of blue → blucket...
2. Perseveration of a phoneme - single thong → single song
3. Transposition of phonemes - silver thistles → thilver sistles
4. Substitution of a phoneme - sow's snout → ... shout
5. Word error - blood → bug
6. Feature error - buggy bumpers → bunky bumpers

Thus far, it appears that slips of the mind resemble slips of the tongue in that the same types of errors are made. The next question to ask is whether the frequencies of the error types are the same. In Table 3 obtained slips of the tongue and mind are compared with respect to frequency of anticipation, perseveration, and transposition. For both slips of the mind and slips of the tongue anticipations predominate, and transpositions are the least common.

TABLE 3
Percentages of error types for slips of the mind and slips of the tongue

	Anticipation	Perseveration	Transposition	Others
Slips of the mind	60%	25%	5%	10%
Slips of the tongue	50%	18%	9%	23%

If one accepts Anan'ev's view that inner speech is characterized by initial consonants only, one might expect slips of the mind to differ from slips of the tongue. In particular one would expect slips of the mind to involve word-initial consonants and clusters to a greater extent than slips of the tongue. As shown in Table 4 most slips of the mind did involve initial consonants or clusters, but this tendency is certainly no larger than that in slips of the tongue.

TABLE 4

Percentage of errors involving word-initial consonants and clusters (I) and non-initial consonants and clusters (N)

	I	N
Slips of the Mind	69%	31%
Slips of the Tongue	78%	22%

The general picture that emerges in these comparisons is that slips of the mind and tongue are no different in any of their properties. Admittedly, this conclusion is based on little data. Nevertheless, I would like to discuss this finding with respect to two issues: the nature of inner speech, and the causes of slips of the tongue.

The speech-like nature of slips of the mind leads to the conclusion that inner speech is similar in form to overt speech and thus involves the same psychological processes. I am not saying that inner speech is always similar to full-blown speech, only that it can be. Undoubtedly, inner speech is fragmentary in some situations, but I would propose that this fragmentariness is not a defining feature.

The final conclusion concerns slips of the tongue. If slips of the mind are identical to slips of the tongue, then it can be stated that overt or sound-producing articulation plays no causal role in slips of the tongue. So slips of the tongue-contrary to their name - are caused by processes occurring in the mental planning of speech and not in the musculature of the articulatory organs. In this way slips of the tongue are really slips of the mind. The only difference is that we can hear them.

REFERENCES

Fromkin, V.A. The non-anomalous nature of anomalous utterances. Language, 1971, 47 (1), 27-52.

Sokolov, A.N. Inner Speech and Thought. New York: Plenum Press, 1972.

Vygotskii, L.S. Thought and Language. Cambridge, Mass.: M.I.T. Press, 1962

Watson, J. Psychology from the Standpoint of a Behaviorist. London: G. B. Lippincott, 1919.

PSYCHOLINGUISTIC EXPERIMENTS IN AMBIGUITY:
ANALOGY, ANOMALY, OR ARTIFACT?

Joseph F. Kess and Ronald A. Hoppe
University of Victoria

The last decade has seen vigorous psycholinguistic interest in questions raised by the nature of contemporary linguistic theory, and ambiguity is a concept which has played a key role in the development of certain syntactic aspects of this theory. It has been claimed that phrase structure grammars operating on surface clues alone cannot provide a proper bracketing for certain ambiguous sentences, and that a deep structure must also be postulated. Such ambiguous sentences can be said to have two distinct underlying deep structures and can be characterized by different syntactic interpretations (readings) at this level, even though they may overlap completely in terms of their surface manifestations. Ambiguity has thus been seen as a keystone in the rationale for postulating a level of deep structure. The questions which then arise relate to what the nature of ambiguity is, how widespread it is, and what it can tell us of competence and performance in terms of producing and processing sentences. One important question, of course, is whether the processing of ambiguity is analogous in any significant way to the processing of sentences in general or whether it is an anomaly in one's general linguistic experience with sentences. Is ambiguity analogous or anomalous as a semantic or syntactic category? Is it a linguistic exercise that one has little difficulty with or cognizance of, be it overtly or covertly? Even more importantly, are psycholinguistic experiments investigating ambiguity really elucidating information about real linguistic abilities, or are the results just artifacts of artificial experimental settings. Should we consider the ambiguity results as being informative about language abilities, uninformative about such abilities, or simply commentaries on extra-linguistic tasks that experiments may pose. To put it another way, as analogy, anomaly, or artifact?

If one agrees with Johnson-Laird's (1974:135) assessment that the fundamental problem in psycholinguistics is simply "what happens when we understand sentences," then it seems obvious that any investigation into aspects of sentence understanding will throw light on the entire linguistic process involved in communication. However, the investigation of ambiguity raises an important consideration. Does such psychological experimentation not raise the question of how reasonable it is to generalize to real-life linguistic activity from experiments which involve extremely contrived tasks in artificial settings? Such approaches may examine behaviours that are far removed from natural responses

to ordinary language.

Some assume that certain tasks like the completion of ambiguous sentences (MacKay, 1966) or the search for and recognition of ambiguous sentences (see MacKay & Bever, 1967) do in fact reveal something about the nature of language. It has been claimed that such experiments may also establish the relative orders of difficulty in processing ambiguities and by extension, may give us some idea of the order linguistic levels are processed in. There is a problem with these assumptions in that any psychological experiment is artificial to a degree, and psycholinguistic experiments are no exception. Several critics (e.g. Adair, 1973) have suggested that the subject in many psychological experiments may adopt a problem-solving attitude, an attitude which is quite different from the one he uses in his normal activities. This certainly may be the case in experimental studies of ambiguity which do not tax normal linguistic abilities, but rather impose an interesting constraint on what possible strategies may be evolved to cope with these tasks. Such strategies may be far removed from the normal tasks speakers perform in actual discourse. It is true that the abilities involved do have an important linguistic input, but in the absence of the normal semantic and pragmatic cues, subjects may employ any variety of techniques to handle such problems. For example, if "prolonged experiences with ambiguous sentences in an experiment can obliterate or distort effects" (Carey, Mehler, and Bever, 1970:254) after the first ambiguous sentence, then we may be testing something as simple as the ambiguity quotient of various sentences or sentence types. Like the Gestalt visual perception experiments with vase-faces, Necker cubes, and duck-rabbits, the "aha" experience of seeing the ambiguity may serve to facilitate or block further accessibility of the two possibilities. It may be premature to speak of levels of processual difficulty across languages. We may be testing for strategies speaker-hearers evolve to cope with an experimental setting in which unusual tasks are presented to them and which they are then expected to handle as if they were common tasks.

Another problem with the experiments on ambiguity concerns the behaviours evaluated in the experiments. Can we say that judging when a phoneme occurred in a sentence (Foss, 1970), completing part of a sentence (MacKay, 1966), or saying whether a sentence is ambiguous or not (Bever, MacKay, 1967; Olson and MacKay, 1970) have anything at all to do with the activities that take place in ordinary discourse? The behaviours examined in experiments, like the tasks sampled in them, may be such an atypical subset of all the language behaviours and language settings as to be construed as simply a silly question receiving varied and even perhaps silly answers.

Most excursions into the problem of ambiguity pay some homage to the fact that ambiguity as such seems to be rarely noticed by hearers of a language. There are too many other seman-

tic, syntactic, pragmatic, environmental and inferential cues for potentially ambiguous sentences to pose any problem to individuals.

Many admit that "the explicit detection of ambiguity is an unnatural process" (Carey, Mehler, and Bever, 1970), and some investigators even suggest that there is hardly a sentence which is not to some degree ambiguous (see Garrett, 1970). However, it is possible that ambiguity may exist everywhere and yet seldom be detected. Psychologically, it may be similar to the cues for depth perception. Our vision is three-dimensional, but we are usually unaware of the cues producing this perception. It took Leonardo to point most of them out to us, and it is only the trained artist who can analyze his three-dimensional perception into the pertinent two-dimensional cues.

The question, of course, is whether individuals process a single reading for ambiguous sentences (the unity theory) or whether they compute all the readings of the ambiguous sentence (the multiple-reading theory) and then decide on one reading as the appropriate one. It is difficult to decide on the correct position without taking into account semantic, pragmatic, environmental, inferential, and contextual considerations which must be true for any given sentence in a discourse setting. Sentences do not operate in a vacuum, nor do conversations take place in isolation. Even in those instances where sentences can be set apart and analyzed alone, domain ties with all the aforementioned considerations must be recognized to exist, no matter what steps are taken to prevent their appearance. It seems only logical that such notions must be taken into account in the resolution of the ambiguity problem, if the answer to the problem is not to be an artificial one and is to have some consequence.

Some experiments have, of course, taken this approach and have either set subjects to perceive sentences in one way by providing another sentential structure or have in fact provided context. For example, an interesting report (Suls and Weisberg, 1970) employs intrasentence word associations to investigate the psychological organization of syntactically ambiguous sentences. An ambiguous sentence like The doctor saw the old Indian dance was preceded by two to four unambiguous sentences as context, which made one of the two possible interpretations highly probable. The prediction was that the pattern of intrasentence word associations would conform to the reading of the sentence which conformed to that called for by the context. The results clearly fulfill the prediction for 14 of the 16 stimuli readings, and underscore the importance of context in contributing to our understanding of sentences.

Another source of difficulty is a practical one. Some of the sentences which have been posed as ambiguous are so only in a restricted or unlikely sense, and some sentences may not be ambiguous at all. Take for example a sentence fragment like

Although Bob Hope frequently delivered delightfully serious speeches (from MacKay, 1966). This sentence supposedly has two readings, one in which Hope delivers very pleasant serious speeches and the other in which he delivers serious speeches frequently and in a delightful manner. One would likely reject outright the ambiguity of this sentence fragment since the second reading is seen only in a strained sense.

One would also expect experimental investigations to be careful in choosing sentences that are only doubly ambiguous. Otherwise, one might be able to attribute some of the results to the fact of multiple ambiguity. For example, a sentence like While lecturing on their stand in Leningrad is in fact multiply ambiguous, but in more than just the two-way lexical and surface fashion MacKay (1966:436) suggests. The sentence is multiply ambiguous (12 ways), possibly more than the experimenter had probably intended. Such multiplicity runs contrary to the definition provided by MacKay and Bever (1967:193) in a contemporaneous paper of the period, where ambiguity is spoken of as "any stimulus pattern which is capable of two and only two distinct interpretations." The multiplicity of meaning does cause a problem for the operational definitions used in experimental studies. However, in the studies by MacKay and his associates where differences among various types of ambiguity were found, there is no indication that the multiplicity of meaning varied directly with the order of the differences. Therefore, the variability which might have been produced by the multiplicity would have made it less likely to find the differences which were obtained in the studies, so the conclusions of these studies are not jeopardized.

Lastly, in trying to make up ambiguous sentences for two non-Indo-European structures, Japanese and Tagalog, we have found one cannot simply issue an order for 20 ambiguous sentences 8 words in length. Putting an artificial constraint on the number of words in such sentences makes for artificial constraints in terms of the type of sentence which will be constructed. This was true for Japanese and Tagalog, and we conjecture that the same is true for English. Perhaps this is why some of the sentences in the experimental investigations of the past have been strained; and yet, if one does not honor this constraint the variable sentence length may affect the results.

In trying to create ambiguity on the surface and underlying structural levels, we found that several structural types favored such ambiguity. The implication here is that ambiguity occurs particularly in a restricted set of syntactic structures. In real speech some of these may be disambiguated by intonation. For example, a popular form is the left-branching modificational pattern shown in the cute child's dress, paralleled by the Japanese kawaii kodomo no yooofuku.

The experimental results on ambiguity to date have

simply been equivocal, with some investigations finding significant differences in the way ambiguous sentences are handled and some not finding any. In those experiments which do find differences, these are not always uniform across results and the nature of the differences seems unclear. Some attempt to answer this seeming dichotomy in the patterning of results has been offered by Bever, Garrett, and Hurtig (1973), but it remains to be seen what the nature of the difference between ambiguous and unambiguous sentence processing is. The claim that the effects seem to be somewhat different depending on ambiguity type is also at this point unclear. Earlier studies (MacKay, 1966, and Bever and MacKay, 1967) had noted differences between the three levels of lexical, surface, and underlying structure ambiguities. However, we should now admit that not only are we unsure of the differences between the effects of each of these ambiguities, but we are also uncertain that there will be differences between these ambiguities in different languages. For example, in Japanese the simple category of lexical ambiguity seems to be subdivided into two types. Japanese exhibits a kanji or character writing system in writing roots and stems, and thus one may have lexical ambiguity where homonyms are represented by two different characters or character combinations or lexical ambiguity with a single character or syllabic representation for a polysemous lexical item. Thus, at least one type of ambiguity may not be the same from language to language in terms of the constraints which may affect the category. It also suggests that so far we may only know something about ambiguity in English and a few related languages like Dutch and French but not much about ambiguity in general.

But, let us argue for the moment, that both syntactic readings of an ambiguous sentence are present during its processing stages. Some experiments have given substantial support to this position, with perhaps the strongest evidence arising from Lackner and Garret's (1972) dichotic simultaneity investigation. Let us also assume that Garrett (1970) is right in assuming that EVERY sentence is ambiguous. This still does not seem to pose any immediate problem for speakers of a language, and even in those studies that did note differences between ambiguous and unambiguous sentences, it was obvious that these caused no problem whatsoever. Indeed, the most amazing thing about the entire paradigm of experiments is that they underscore the lack of difficulty that any normal native speaker of the language has with ambiguity. It seems the problem is more one for the linguistic scholar, trying to map three-dimensional communicational realities onto two-dimensional branching-tree diagrams.

Linguists recognize this when dealing with computer systems being instructed in the niceties of human language. Winograd (1972), for example, notes that we should not expect a computer to deal reasonably with language unless it understands the subject it is discussing. Obviously, when human beings see

or hear sentences they make use of their encyclopedic storehouse of knowledge and experience, and any and all immediate pragmatic cues to understand it. Some of the same must be included in the instructional program provided for a computer system which is designed to understand natural language. The study of ambiguity in isolation does tell us some interesting things, but are they the things that we want to know and that we can make use of? Sooner or later we will have to face how it is that sentential and pragmatic context affects comprehension and our mechanical friends' needs seem to point to just that.

What will have to be decided is whether ambiguity is true of any or all sentences, as Garrett (1970) has claimed. His suggestion is that normal sentence processing must deal routinely with ambiguity because there is simply so much of it. All sentences in his reckoning can be thought of as a final choice among several possible structural or lexical interpretations. However, many of these interpretations must be thought of as being applicable only in extended contexts. For example, Garrett's sentence John's uncle showed him a whole roomful of walking sticks can be given a reasonable interpretation by talking of cobblers who, like Geppetto, are given to making their wooden creations take on bipedality. Not your average first interpretation, but nonetheless, simple and acceptable enough, and the remarkable plasticity of language is its ability to create like this. Should we, then, talk about first orders of priority instead, as we have done on the level of semantic features, and discount certain collocations as extensions or metaphors? But we run immediately into the same problem as we did with semantic features -- what is a metaphor and by whose decision?

After all, inherent vagueness is a fact of linguistic life for isolated sentences. There are too many things to say, and only limited linguistic means of saying them. Sentences in isolation are often vague and we depend upon context for everything. For example, the Japanese sentence Kimasu can mean a variety of things: He is coming; She is coming; It is coming; The train is coming; We are coming; and on and on. So does the English sentence It is here. The mind is bogged by the readings this sentence can stand for in terms of what 'it' is and where 'here' is. Possibly what is informative about ambiguity is what it may tell us about sentence processing mechanisms, not because it is a subset of the category 'normal sentence', but rather because so-called normal sentences are often vague, ambiguous, or both.

Perhaps some of the more interesting questions that we should be pursuing deal with how it is that ambiguity is resolved. Considering that ambiguity threatens to overwhelm us with its ubiquity, but seldom does, how is it that speakers easily and automatically resolve this fact? Does it at anytime truly present problems though? Where? If the answer to this last question is

affirmative, the answers to the other questions are obviously of great practical import in matters pedagogical. Very simply, it seems at this point that ambiguity is a very general phenomenon, and yet is rarely noticed. We also know that in terms of the psycholinguistic tasks designed to date some results have shown that differences do exist in the performance associated with sentences said to be ambiguous and those said to be normal; some results have not. All are, however, agreed that ambiguity is not a problematic source of difficulty for speakers. What is it then that makes it NOT a problem?

If almost every sentence we process is potentially ambiguous, then the study of ambiguity may be a useful tool enabling us to get a better grasp on sentence processing. When processing sentences in general, do we have a probability hierarchy of meanings which we use in our understanding, and how do the hierarchy and the selection from it vary with the pre- and post-sentences as well as pragmatic aspects of the situation? These are proper questions for future research and ambiguity is best thought of as an analogy to general language processing.

References

- Adair, J.G. (1973) The Human Subject: The Social Psychology of the Psychological Experiment. Little, Brown and Company, Boston.
- Bever, T.G., Garrett, M.F., and Hurtig, R. (1973) The interaction of perceptual processes and ambiguous sentences. Memory and Cognition, 1 (3) 277-286.
- Carey, P.W., Mehler, J., and Bever, T.G. (1970). Judging the veracity of ambiguous sentences. J. Verb. Learn. Verb. Behav. 9. 243-254.
- Foss, D.J. (1970). Some effects of ambiguity upon sentence comprehension. J. Verb. Learn. Verb. Behav. 9. 699-706.
- Foss, D.J., and Jenkins, C.M. (1973). Some effects of context on the comprehension of ambiguous sentences. J. Verb. Learn. Verb. Behav. 12. 577-589.
- Garrett, M.F. (1970). Does ambiguity complicate the perception of sentences? In Flores D'Arcais, G.B., and Levelt, W.J.M., Eds., Advances in Psycholinguistics, North-Holland Publishing Co., Amsterdam.
- Hogaboam, T.W., and Perfetti, C.A. (1975). Lexical ambiguity and sentence comprehension. J. Verb. Learn. Verb. Behav. 14. 265-274.

- Householder, F.W. (1973). On arguments from asterisks. *Foundations of Language* 10. 365-376.
- Johnson-Laird, P. (1974). Experimental psycholinguistics. *Annual Review of Psychology* 25. 135-160.
- Lackner, J.R., and Garrett, M.F. (1972). Resolving ambiguity: effects of biasing context in the unattended ear. *Cognition* 1. 359-372.
- MacKay, D.C. (1966). To end ambiguous sentences. *Perception and Psychophysics* 1. 426-436.
- MacKay, D.G., and Bever, T.G. (1967). In search of ambiguity. *Perception and Psychophysics* 2. 193-200.
- Miller, G.A. (1962). Some psychological studies of grammar. *Amer. J. Psych.* 17. 748-762.
- Mistler-Lachman, J.L. (1972). Levels of comprehension in processing of normal and ambiguous sentences. *J. Verb. Learn. and Verb. Behav.* 11. 614-623.
- Osgood, C.E. (1971). Where do sentences come from? In Steinberg, D.D., and Jakobovits, L.A., Eds., *Semantics: An Interdisciplinary Reader in Philosophy, Linguistics, and Psychology*, Cambridge University Press, New York.
- Olson, J.N., and MacKay, D.G. (1974). Completion and verification of ambiguous sentences. *J. Verb. Learn. Verb. Behav.* 13. 457 - 470.
- Perfetti, C.A., and Goodman, D. (1970). Semantic constraint on the decoding of ambiguous words. *J. Exp. Psych.* 86. 420 - 427.
- Piquette, E. (1976). The translator's sensitivity to syntactic ambiguity: a psycholinguistics experiment. Can. J. Ling. 21. 95 - 106.
- Winograd, T. (1972). Understanding natural language. *Cognitive Psych.* 3. 1 - 191.

TOWARD AN INTEGRATION OF PIAGET AND VYGOTSKY: A
CROSS-CULTURAL REPLICATION (FRANCE, GERMANY, CANADA)
CONCERNING COGNITIVE CONSEQUENCES OF BILINGUALITY⁽¹⁾

Bruce Bain

Agnes Yu

University of Alberta

Bain (1975b) offered a tentative proposal concerning an approach which could be profitably pursued in quest of a comprehensive theory of cognitive development. The purpose of this paper is essentially to replicate Bain's (1975b) procedures to see if the approach can stand that type of scrutiny inherent in a cross-cultural study. For a detailed description of those theoretical and research concerns, see, Bain (1975b). Only a brief overview is presented here.

Piaget and Vygotsky

Bain (1974a,b; 1975a,b; 1976; 1977) following Schmidt (1973), suggests that speech, seen in the semiotic sense of "a tool, and, a product of social conditions", is an avenue along which a useful integration of the cognitive theses of Piaget and Vygotsky might proceed. Piaget's thesis (1923, *et passim*) describes human development as a progressive, epigenetic unfolding, one in which, if all contributing aliments are present, there will be a sequential emergence of stages; each stage being of increased complexity of logic, eventually ending in a stage of a generalizable epistemic competence. Piaget holds that speech follows this evolution, that is, speech plays no instrumental role in initiating or propelling a child through these stages, but, rather, it simply reflects the relative complexity of each stage. Vygotsky's thesis (1928, *et passim*) describes human development as a progressive internalization of the social-material base of a child's particular existential situation. Vygotsky holds that this process is mediated by all manner of semiotic signs, but that the sign par excellence is human speech. In other words, Vygotsky conceives of speech as being inextricably interwoven with historical, cultural experience, and, simultaneously, as playing a leading role in initiating and directing the development of the child. The development of the higher cognitive functions is, for Vygotsky, contingent upon the child's mastery of speech.

Redirection

Bain (1975b; *esp.*) offered some direction to a possible synthesis of Piaget's "biological imperative thesis" with Vygotsky's "sociogenesis thesis" by re-examining an old issue in psychology,

namely, differences in cognitive processes between unilinguals and bilinguals; and, by interpreting the results in light of the strengths of each thesis. He compared performances of unilingual (English or French) and bilingual (English and French) Canadian school children on two types of cognitive tasks, one contemplative and one participative. Each type of task places a somewhat different demand on the knower's cognitive processes. A contemplative task demands suspension of ongoing activity in favour of reflection, inspection and logical analysis. A participative task demands a more-or-less immediate, organismic response to the physiognomic properties of an object. Profiles based upon task performance of both types of processes tend to provide a more balanced picture of the knower's cognitive abilities.

The methodology designed to assess contemplative performance concerned: (1) presenting a series of Gagné-type (1961) "discover the rule" problems in one testing session and, transferring the learned rule to solve another series of like problems in a second testing session; (2) to two pairs of matched groups of children; (3) one pair matched at preoperations, one at concrete operations; (4) all were matched for I.Q., S.E.S., school grades; (5) one group of each pair were balanced bilinguals, the other were unilinguals. A principal assumption of the original study was that speech qua speech had been reasonably isolated from the constellation of influencing variables. The major results were: (1) at the preoperational stage, the bilingual group discovered the rule significantly faster ($p < .05$) than the unilingual group; (2) at the concrete operational stage, there was no significant difference in discovery time, although the trend ($p < .08$) was in favour of the bilingual group; (3) there were no differences in transfer time at either stage. The latter result of "no difference in transfer time at either stage" was seen in terms of the Piagetian view that ability to generalize, that is, to use or to apply previously acquired schemata, is much the same for all children at the same stage of development regardless of speech type. However, the result of "significant difference in discovery time", particularly at the earlier stage, was seen in terms of the Vygotskian view that differentially organized speech semiotic results in differential cognitive consequences. In other words, along with Vygotsky, it was suggested that the bilingual child, because of a more flexible speech system, was better able to use his speech to mediate the discovery side of the analytical process; while along with Piaget, it was suggested that a child at a given stage of contemplative cognition, regardless of cultural origins or merits of certain aliment that helped him get there in the first place, is only able to use acquired knowledge with an equivalent facility to his stage mates.

The methodology designed to assess participative performance concerned: (1) presenting The Portrait Sensitivity Test (Bain, 1973); (2) to unilingual (English or French) and balanced bilingual (English and French) Canadian children, ages six and eleven.

The major result was that, at both age levels, the bilingual children made fewer classification errors ($p < .05$, $p < .01$; respectively) than their unilingual age mates. The result of "a progressive sophistication in ability to creatively respond to the demand characteristics of physiognomic expression" was seen in terms of the Vygotskian view that qualitative difference in speech systems result in differential ability to mediate all manner of semiotic signs. The Piagetian view, not lending itself to a parsimonious interpretation of these participative type phenomenon, was not utilized in these regards.

REPLICATION: CANADA(OVERSEAS),FRANCE,GERMANY

Cross-cultural studies are fraught with comparability problems. This replication is a little less problematic in that the S populations are of a more or less common historical and cultural ancestry. However, even though the original design features were, in the main, studiously maintained, certain modifications were necessary because of local conditions. These are described below.

Contemplative Task

Subjects

The Ss were all school children selected from three different populations: (1) native born French living in the Alsace; (2) native born Germans living in the lower Rhine; (3) native born Canadians living in the lower Rhine, attending The Canadian Armed Forces Schools Overseas. The selection procedures for the overseas Canadians were as per the original (1975b) study. That is: (1) Ss' level of epistemic competence, preoperations or concrete operations, were determined as per Goldschmid and Bentler (1968); (2) Ss were designated "balanced bilingual", that is, equal facility in both languages, if they scored between 4 and 5 on the three 5-point scales: (i) home interview, scored by parents; (ii) teacher interview, scored by teacher; (iii) self report, scored by individual S. A S was designated unilingual if he could not respond verbally or behaviourally to a simple prescriptive statement in the other language of his particular speech community, or in any other second language. The major modification for French and German Ss concerned S.E.S. criterion. Instead of Blishen's scale, which was used for the Canadian samples, a teacher report of "petit bourgeois" was taken as being more-or-less comparable for the French and German samples. Other selection procedures were identical.

The final sample breakdown for French Ss consisted of: (1) at the preoperational level, 5 unilingual French speakers, 5 unilingual Alsatian speakers, 10 bilingual French/Alsatian speakers; (2) at the concrete operational level, 10 unilingual

French speakers, 10 bilingual French/Alsatian speakers. It was not possible to locate concrete operational Ss who spoke Alsatian only. By and large children of Alsatian origins, to greater or lesser degrees, speak both languages by the time they have reached concrete operations. It was not possible to locate the opposite combination, that is, children of French origin living in the Alsace who speak Alsatian. The final sample breakdown for German Ss consisted of: (1) at the preoperational level, 10 unilingual German speakers, 10 bilingual German/English speakers; (2) at the concrete operational level, 10 unilingual German speakers, 10 bilingual German/English speakers. It was not possible to locate native born Germans who spoke English only. The final breakdown for overseas Canadian Ss consisted of: (1) at the preoperational level, 5 unilingual Canadian-English speakers, 5 unilingual Canadian-French speakers, 10 bilingual Canadian-English/French speakers; (2) at the concrete operational level, 5 unilingual Canadian-English speakers, 5 unilingual Canadian-French speakers, 10 bilingual Canadian-English/French speakers. For all groups, at both stages, there was an equal breakdown by sex.

Procedure

The paradigm for the tasks used in this study was to discover rules for finding the sum of a series of numbers. For example:

- A: 1, 3, 7, 15, 31, ____.
 B: $1/2$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3, ____.
 C: 1, 2, 4, 8, 16, ____.

Following initial trials on a pilot group of concrete operational children, the original pool of 120 test items was reduced to 50 items of equal difficulty (split-half, $r = .88$), randomly divided into 2 lots of 25 each. Twenty-five items per testing session was the most reliable number for the concrete operational children. From an identical procedure with a pilot group of preoperational children, 30 items of equal difficulty (split-half, $r = .81$) were obtained, and randomly divided into 2 lots of 15 each. Fifteen items per testing session was the most reliable number for preoperational children.

Each S was presented with one appropriate lot on one day, and the second after a delay of two days. The items within the lots were randomized following each session. The usual precautions of time of day and fatigue were taken into consideration. Subjects worked individually. They were presented with one item at a time. They were told "to complete one item before going on to the next"; that they "had 10 minutes to complete each item"; and "to work as fast as possible". If a S made an error he was simply told "to look at it again". At the outset, the S was told to find the rule that would help him solve the problem. "Do it by yourself. You will not be given any hints." At the second session, the S was

told "to use the rules he learnt last day to help solve the problems". The remainder of the original instructions were also repeated. In both testing sessions, Ss had to complete all items correctly. If a S took longer than 10 minutes to complete an item he was rejected from the study. Time required to complete each lot was the performance measure. The procedures began with each S being asked to designate the language in which he wished to be given the instructions. All subsequent communications were carried on in the designated language.

Results

Table 1 presents the means and standard deviations in times required to discover rules, to transfer rules and total time; by level of operations; by linguality; within each community; in comparison with original findings. Note the similarity of pattern of findings cross-culturally. In all cases, within each community: (1) the bilingual preoperational Ss discovered the rules significantly more rapidly than did their unilingual stage mates; (2) trend only, not real differences, were found for discovery times at the concrete operations stage; (3) there were no differences in transfer time.

Table 2 presents the profile of significant differences between and within each community; by level of operations; by linguality. At the concrete operations level there were no (.05/.01) significant differences between communities or between type of speech by community. At the preoperations level there were: (1) no differences between bilinguals by community; (2) no differences between unilinguals by community; (3) significant differences between all bilingual groups and all unilingual groups regardless of community.

Discussion

Three points stand out consistently. The first is that there seems to be a pyramidal effect of speech qua speech in mediating the acquisition of cognitive rules. The lower the stage the larger the effect; with speech becoming less important as a lure to cognition or as a guide to attention at the higher stages. Whether or not this finding is an artifact of these task procedures remains to be investigated. The second point is that cultural experience, in the broader, anthropological sense apparently plays little role in the cognitive demands of this contemplative type of epistemic task. Cultural experience notwithstanding, a child's level of contemplative competency is his level of contemplative competency. The third point is that the least poor explanation of these findings is perhaps best given from a synthesis of Piagetian and Vygotskian points of view. In other words, a judicious interpretation of the leading role of

TABLE 1
WITHIN COUNTRY COMPARISON OF UNILINGUAL AND BILINGUAL PERFORMANCE ON CONTEMPLATIVE TASK

Level of Operations	Country	Linguality	N	Time in Minutes ¹						Total
				Discovery		Transfer				
				M	SD	M	SD			
Concrete Operations	France	Biling.	10	31.09	9.37	28.72	7.90			59.81
		Uniling.	10	39.69	10.69	28.83	9.31			68.52
	Germany	Biling.	10	30.80	8.33	29.43	8.96			60.23
		Uniling.	10	37.83	10.93	30.01	9.42			67.84
	Overseas	Biling.	10	30.91	8.21	29.95	9.65			60.86
		Uniling.	10	39.95	11.06	28.31	8.31			68.26
	Mainland ²	Biling.	10	31.25	9.38	28.53	8.55			60.18
		Uniling.	10	39.48	11.84	29.02	8.70			68.15
Preoperations	France	Biling.	10	26.24	9.01	24.15	7.59			50.39
		Uniling.	10	36.15	8.68	24.62	8.06			60.77
	Germany	Biling.	10	25.32**	8.43	25.01	9.10			50.33
		Uniling.	10	35.94	8.64	23.60	7.56			59.54
	Overseas	Biling.	10	25.90**	9.39	25.12	8.12			51.02
		Uniling.	10	36.01	8.03	24.66	7.34			60.07
	Mainland ²	Biling.	10	26.25**	8.50	24.01	7.72			50.26
		Uniling.	10	37.35	9.46	24.16	7.53			61.51

** (p < .05)

1. Longer time = slower performance. Total times cannot be compared between levels because of different number of task items.
2. Original findings(1975b).

TABLE 2
BETWEEN AND WITHIN COUNTRY COMPARISON OF UNILINGUAL AND BILINGUAL PERFORMANCE ON CONTEMPLATIVE TASK

Level of Operations	France		Germany		Overseas Canadians		Mainland ¹ Canadians	
	Biling.	Uniling.	Biling.	Uniling.	Biling.	Uniling.	Biling.	Uniling.
Concrete Operations	France	-	1.91	.07	1.49	.05	1.93	.04
		Biling.	-	2.08	.39	2.06	.05	1.88
		Uniling.	-	-	1.62	.03	2.09	.11
	Germany				-	1.60	.43	1.44
		Biling.				-	2.07	.09
		Uniling.				-	-	1.89
Preoperations	Overseas Canadians							
		Biling.						
		Uniling.						
	Mainland ¹ Canadians							
		Biling.						
		Uniling.						
Concrete Operations	France	-	2.51**	.05	2.46**	.04	2.51**	.30
		Biling.	-	2.70**	.05	2.76**	.04	2.86**
		Uniling.	-	-	2.65**	.02	2.91*	.35
	Germany				-	2.73**	.02	2.52**
		Biling.				-	2.56**	.34
		Uniling.				-	-	2.58**
Preoperations	Overseas Canadians							
		Biling.						
		Uniling.						
	Mainland ¹ Canadians							
		Biling.						
		Uniling.						

p < .05, df 18, t = 2.10**
p < .01, df 18, t = 2.88*
¹ Original findings(1975b)

speech in the acquisition or assimilation of logical problems is provided by the Vygotskian thesis; while Piaget's thesis does more justice to the following role of speech in the use of accommodated knowledge.

Participative Task

Subjects

The S.E.S. and linguality criteria, and their determination, were the same as noted above. Age was the only other criterion in this study. At the younger age, no S was less than 6y0m nor more than 7y0m. At the older age, no S was less than 11y0m nor more than 12y0m. These Ss were not matched for school grades and stage of epistemic competence.

The final sample breakdown for French Ss consisted of: (1) at the younger age, 25 unilingual French speakers, 28 unilingual Alsatian speakers, 50 bilingual French/Alsatian speakers; (2) at the older age, 50 unilingual French speakers, 50 bilingual French/Alsatian speakers. The final sample breakdown for German Ss consisted of: (1) at the younger age, 50 unilingual German speakers, 36 bilingual German/English speakers; (2) at the older age, 50 unilingual German speakers, 32 bilingual German/English speakers. The final sample breakdown for the Overseas Canadian Ss consisted of: (1) at the younger age, 25 unilingual Canadian-French speakers, 25 unilingual Canadian-English speakers, 50 bilingual Canadian-French/English speakers; (2) at the older age, 25 unilingual Canadian-French speakers, 25 unilingual Canadian-English speakers, 50 bilingual Canadian-French/English speakers. For all groups, at both age levels, there was an equal breakdown by sex.

Procedure

The Portrait Sensitivity Test was constructed by Bain (1973) to test participative cognition. It consists of 24 black and white reproductions of classic portraits, painted by masters such as Durer, Hals, and Rembrandt. Each portrait expresses one dominant emotion. Based on artists' report, critics' report, and a norming study ($r = .86$) each portrait was classified as predominantly expressing either Love or Surprise or Contempt or Anger or Fear. The portraits are mounted on individual film slides for projection onto a screen. A checklist consisting of the 5 categories of emotion stated on the horizontal and 24 numbered blank spaces on the vertical is provided. The category headings were directly translated into the languages concerned.

Small groups of children were seated in front of a projection screen. A few preliminary slides were shown to ensure that the Ss understood what was expected of them; to adjust their seating position, etc.. The portraits were shown one at a time. No specific time criterion was set for completion of a slide. The Ss were simply queried after some seconds whether or not they had finished with the portrait. Each S filled in his own checklist without assistance. The instructions given were:

Do you see this face on the screen? I want you to tell me how you would feel if you felt the way this face feels. If you would feel 'love' then check-off love; if you would feel 'surprise'...etc.. Check-off only one feeling per slide. The first feeling you get when you see the face is the one I want you to put down. Try to do each one as fast as you can.

The instructions, particularly concerning speed, were periodically repeated during the presentation. The procedures began with each S being asked to designate the language for his checklist and instructions. All subsequent communications were carried on accordingly.

Results

Table 3 presents the means and standard deviations of number of correct classifications; by age level; by linguality; by community; in comparison with original findings. Note the similarity of pattern of findings cross-culturally. In all cases, within each community: (1) the bilinguals performed significantly better than their unilingual age mates; (2) there was a general increase in level of significance from the younger to older bilingual groups.

Table 4 presents the profile of significant differences between and within communities; by age; by linguality. At the younger age: (1) there were no differences between unilingual groups; (2) there were no differences between bilingual groups, although the levels of significance were slightly higher for the two European bilingual groups. This level of significance differential suggests: (1) that the European bilingual groups performed significantly higher than the Canadian unilingual groups; (2) while there was no significant difference between the Canadian bilingual groups and the European unilingual groups. At the older age: (1) there were no differences between unilingual groups; (2) both Canadian bilingual groups performed significantly higher than both European unilingual groups; (3) both European bilingual groups performed significantly higher than the Canadian unilingual groups and the bilingual groups.

TABLE 3
WITHIN COUNTRY COMPARISON OF UNILINGUAL AND BILINGUAL PERFORMANCE ON PARTICIPATIVE TASK

Age	Country	Linguality	N	Correct Classifications ¹		
				M	SD	
11	France	Biling.	50	13.54*	2.12	
		Uniling.	50	8.46	3.31	
	Germany	Biling.	32	12.69*	3.21	
		Uniling.	50	8.72	2.26	
	Overseas	Biling.	50	11.30*	2.93	
		Uniling.	50	8.33	3.15	
	Mainland ²	Biling.	42	11.12*	2.07	
		Uniling.	42	8.64	2.27	
6	France	Biling.	50	8.96**	4.01	
		Uniling.	53	7.32	2.69	
	Germany	Biling.	36	8.93**	3.26	
		Uniling.	50	7.26	2.99	
	Overseas	Biling.	50	8.16**	2.49	
		Uniling.	50	6.93	3.20	
	Mainland ²	Biling.	60	8.21**	2.65	
		Uniling.	58	7.10	2.93	

* ($p < .01$)

** ($p < .05$)

1. Number correct out of 24.

2. Original findings(1975b).

TABLE 4
BETWEEN AND WITHIN COUNTRY COMPARISON OF UNILINGUAL AND BILINGUAL PERFORMANCE ON PARTICIPATIVE TASK

Age	France		Germany		Overseas Canadians		Mainland ¹ Canadians		
	Biling. Uniling.	Biling. Uniling.	Biling. Uniling.	Biling. Uniling.	Biling. Uniling.	Biling. Uniling.	Biling. Uniling.		
11	France	-	9.07*	1.33	10.95*	4.39*	9.65*	5.50*	10.65*
			-	5.72*	.46	4.58*	.20	4.67*	.31
	Germany			-	6.11*	1.99**	5.97*	2.42**	6.04*
					-	4.96*	.71	5.33*	.17
	Overseas Canadians					-	4.87*	.35	4.93*
	Mainland ¹ Canadians						-	5.07*	.54
								-	5.28*
6	France	-	2.41**	.04	2.39**	1.19	2.78*	1.12	2.70*
			-	2.44**	.11	1.62	.66	1.75	.41
	Germany			-	2.42**	1.17	2.78*	1.11	2.73*
					-	1.64	.53	1.73	.28
	Overseas Canadians					-	2.12**	.10	2.00**
	Mainland ¹ Canadians						-	2.21**	.28
								-	2.13**

p < .05, z = 1.96**

p < .01, z = 2.58*

¹ Original findings (1975b).

Discussion

Four points stand out consistently. The first is that the developmental relationship between speech *qua* speech and participative cognition tends to become more highly intertwined with development; a proposition not unlike that put forward by Werner and Kaplan (1963). Unlike contemplative development, where speech *qua* speech seems to play less of a leading role as development advances, in the participative instance, it comes to play a progressively more influential role as a guide to physiognomic recognition. Whether or not this notion of a progressive intertwining of speech and participative cognition holds true with other materials or in related areas of development remains to be empirically tested. The second point is that unlike contemplative cognition, wherein the logical demands of those kinds of tasks apparently transcend any one culture, the physiognomic demands of a participative task tend to be definitely influenced by cultural experience. The European experience apparently obtains a greater sophistication in participative cognition, particularly with the older children. Moreover, just as the French and German scores were closer to each other, so, too, were the Overseas Canadian and Mainland Canadian profiles closer to each other. This seeming contradiction of Canadians living in Europe, but performing like Mainland Canadians, is accounted for with the recognition that the Overseas Canadians live in a compact Canadian community, attend Canadian schools which have Canadian teachers and Canadian curricula; in effect, they live in cultural Canada transplanted to geographical Europe. The third point, Piaget's contention that contemplation subordinates participation during development, is seriously questioned. Far from withering away as Piaget suggests, participative competencies seem to become more erudite and specific during development. Undoubtedly there is an interaction between these two modes of cognition, but, Werner & Kaplan (1963) and Bain (1973) notwithstanding, the nature of the relationship remains to be fully tested. The fourth point is that a Vygotskian point of view seems best able to be heuristic toward these latter data. It seems that the material-social base of the bilingual, as mediated by his particular type of speech system, allows a unique direction to his participative competency.

CONCLUSION

The general finding of a similar profile of cognitive abilities between unilingual and bilingual children across three different language *cum* cultural communities is central to many concerns. On the theoretical level it suggests that studies which focus on speech, on qualitative differences in speech-as-a-tool-to-serve-cognition; a tool which is itself a product of the vagaries of social conditions, will be useful in unravelling the complexities of human cognition. On the practical level, it suggests that parents and educators should be concerned about

ensuring a bilingual experience for their children. The results of these studies dramatically emphasize the cognitive flexibility accruing from a bilingual experience. Whether or not a formal integration of the theses of Piaget and Vygotsky will be fully useful in these regards remains to be seen. However, the warp and woof of each thesis seem to be of greater heuristic value than either alone.

Note:

- (1) This paper was part of a research project titled "Individual, Language and Society" supported by The Canada Council, grant nos. W740454 & S67-1001. The senior author was a Canada Council Research Fellow, located at l'Université de Strasbourg, during the data collection period. The second author is a doctoral candidate at the University of Alberta. The authors wish to acknowledge the following people for their assistance: Don Vinge & Florent Tremblay (Cdn. Armed Forces Schools Overseas) Andrée Tabouret-Keller & Stephan Jonas (Strasbourg) Robert Haymond (Alberta) Michel Paradis (McGill).

REFERENCES

- Bain, B.C. Toward a theory of perception: participation as a function of body-flexibility. Journal of General Psychology, 1973, 89, 157-296.
- Bain, B.C. Bilingualism and cognition. In S. Carey, Bilingualism, Biculturalism and Education. Edmonton: University of Alberta Press, 1974 (a).
- Bain, B.C. Semiotic in psychology. The Canadian Journal of Research in Semiotics, 1974, 2, 55-61 (b).
- Bain, B.C. A Canadian education. Journal of Canadian Studies, 1975, 10, 57-62 (a).
- Bain, B.C. Toward an integration of Piaget and Vygotsky: bilingual considerations. Linguistics: an International Review, 1975, 160, 5-20 (b).
- Bain, B.C. Verbal regulation of cognitive processes: a replication of Luria's procedures with bilingual and unilingual infants. Child Development, 1976, 47, 543-546.
- Bain, B.C. Langage et perception dans l'éducation de l'enfant. Le journal Canadien de recherche sémiotique, 1976, 1, 41-52.
- Bain, B.C. The Consequence of Unilingualism, Disruptive Bilingualism and Creative Bilingualism for the Development of the Body Schema: a Cross Cultural Study Involving Canada, Italy and (West) Germany (1977, in press).
- Gagné, M. and Brown, L. Some factors in the programming of conceptual learning. Journal of Experimental Psychology, 1961, 62, 313-321.
- Goldschmid, Marcel L. and Bentler, Peter M. Conservation Assessment Kit. San Diego: Educational & Industrial Testing Service, 1968.
- Piaget, J. Le langage et la pensée chez l'enfant. Neuchatel: Niestle, 1923.
- Schmidt, W.H.O. Child Development: the human, cultural and educational context. New York: Harper & Row, 1973.
- Vygotsky, L. Thought and Language. Boston: M.I.T. Press, 1928.
- Werner, H. and Kaplan, B. Symbol Formation. New York: John Wiley & Sons, Inc., 1963.

SOME COMMON TRAITS OF VISUAL AND VERBAL
LANGUAGE AND LANGUAGEING

John L. Debes

Jacob Bronowski said that "all science is a search for unity in hidden likenesses. The scientist looks for order in the appearances of nature by exploring such likenesses." I write you, members of this conference, as scientists skilled in seeking and exploring likenesses in one of the most important of all aspects of nature, human languageing, to engage in a challenging and new exploration, to reveal the likenesses, the parallelisms, the analogical forms and behaviors that exist between the verbal language in which you are so expert, and the emerging visual languages in which no one is expert.

With the advent of that new and omnipresent form of languageing in our society, namely the visual languageing that uses photo- or video-visuals as signs, it has become necessary to propose a less traditional definition of language because, rigorously applied, older definitions "include" visual language "out." Drawing upon traditional definitions, the thoughts of linguistic pioneers, and the work of some LACUS members it is possible to arrive at a point of view and definition that does include visual languageing. However, it is not a definition of language as artifact; instead, it focuses to a larger extent on human behaviors that characterize languageing, first, and only then the sign-artifacts. In other words, human languageing is behavior first, and the surface structure realizations are a consequence of those behaviors and therefore of a secondary or dependent nature.

In what ways does visual languageing using body language or pictorial signs resemble languageing using words? To learn this we need to do three things, first, study the behaviors of people engaged in visual languageing, then second, study the artifact, that is, study visual language and third, restudy verbal language in the light of what has been learned about visual languageing.

Efforts to study visual languageing have been going on for some time. But it was only recently that it became evident that there were some noticeable differences in visual language output of well-established, older, pioneering film and video makers and the language created by young people brought up on TV.

To be brief, the visual language of people who were taught verbal language in their early youth was different than the visual language of people who learned visual language first. The visual language of those brought up with emphasis on verbal language is characterized, generally, by relatively verbal structures and innovations that reflect the logic, or organization of verbal structures or mimic verbal idioms. The visual language of those brought up on TV is, at first, merely an echo of the models they have internalized from the TV tube, but then becomes increasingly mixed with visual expressions that hark back to no very evident verbal counterpart.

So, at this time in development of our more visual culture, it makes sense to study the visual language of the TV generations. For that reason, I will begin by showing you a film created by third graders, children who had never made a film before. The children created their own story, did much of their own filming, and decided on their own sound effects. Their teacher knew little or nothing about filmmaking. They did have a technician who helped them with advice on how to film some things and especially on how to add the sound track. Here it is --

-- The \$2.50 Man --

That's a fun film, isn't it? But this film is full of cliches, conventions and stereotypes. What these young people have done is not only acquire a knowledge of the kind of visual language used on television and in movies, but they have also been able to express themselves in ways uncomfortably like the ways in which we have expressed ourselves. Yes, I said we. It was we that prepared the TV and movie models from which they worked.

That film represents visual languaging in its early phase during which the child mimics the languaging that his culture gives him to mimic. People who have been permitting children to make movies in school say that after the first film, a less conventional, more sophisticated visual language begins to emerge, language that is clearly more independent of the verbal model.

Beginning with this presentation, I am suggesting that we call the study of visual languaging Eidetics. The term was not original with me but was recommended to me by Roger Wescott. My thanks to him and my apologies for having insisted he defend the term when its suitability seems so obvious. Eidetics, I am sure you all know better than I, comes from the Greek word standing for the idea: forms with meaning. I am grateful to the Greeks, also, for what now seems like serendipity.

Languaging is a term I borrowed from my friend Dr. Andrew Hilgartner, a mathematical biologist and philosopher who has created a non-Aristotlean language. By languaging, I mean human languaging, that is, all those intentional behaviors in which visual and/or "verbal" segments from a culture are selected by one human to be offered as signs which are then presented sequentially in culturally-established order (syntax), and in a culturally-appropriate context and time to another human for the purpose of communicating and all those intentional behaviors in which another human listens or reads and understands what is presented.

Languaging behaviors can be regarded as using signs in two ways, evanescent (spoken or acted) and distanced (written, drawn, photographed). Behaviors with respect to the use of signs are analogically related as follows:

Analogical Forms of Languageing Behavior
English/Visuals

Use of Signs	Verbal	Visual
Evanescent	spoken	body (or body + object)
	hearing the above	observing the above
Distanced	writing	composing photos, cartoons
	reading the above	observing the above

In the languageing process that begins with cognition and ends with the surface-structure of the artifact we call language, the commonalities of behavior are numerous and are exactly analogous as could be expected considering the fact that although verbal languageing is managed primarily by the left hemisphere, visual languageing is managed primarily by the right hemisphere. Some of the analogical parallels are indicated in the chart on the screen.

LANGUAGEING	LINGUISTIC "STRATA"	EIDETIC "STRATA"
Realization	"Verbalization"	"Visualization"
Behavior: selecting and ordering	Sememes	Visual Conventions
	Lexemes	Eidemes
	Morphemes	Visemes
	Phonemes	Kinemes
Ordering	Muscular Contractions	
Choosing	Mode, time, audience, context	

At the cognitive level both forms of languageing begin with the perception or conception of a pattern. I am using the word pattern here after Bateson who defined pattern as "ideas and spaces between ideas." From this pattern, we abstract some aspect, thought, experience, or feeling that we might wish to communicate. All of this is at the non-verbal level and by that, I don't mean necessarily visual either. Because we are motivated to communicate, we hunt for and conceive of a metaphor for our idea. That metaphor may be visual, auditory, tactile, olfactory, etc., but it is not yet conceived in terms of signs either visual or verbal. Up to this point we have not been engaged in languageing but in cognition that may or may not be followed by languageing. After all, to communicate our metaphor, we may merely administer a hearty kick and that is not the systematic stuff that characterizes visual and verbal languageing.

Languaging begins when we decide to communicate our metaphor by encoding it in verbal or visual signs ordered according to the languaging rules laid down by our culture. In other words, languaging is intentional, and the choice of mode is intentional, as is the choice of time, the choice of context and the choice of audience.

After that, we proceed along a course that will look familiar to you in its verbal aspect at least. For each of the verbal forms, there seem to be visual forms. But I must recommend your attention to what seems obvious: These likenesses (to use Bronowski's term) are obfuscated by surface structures that are as different as ear and eye. To perceive the likenesses in many cases it is necessary to ask ourselves what is the purpose of this group of shapes, and does it originate from a similar organizational behavior, but merely expressed in another sense realm.

As time permits, I will put on the screen some visuals that seem to me to be properly classified as visual illustrations of some of the list of Distinctive Traits of Language as artifact that appears in the book by Hewes, Stokoe and Wescott, (see page 6).

Behaviors Common to Visual and Verbal Languaging - Mandatory

- . abstracting from some thought or experience
- . "creating" a metaphor for that abstraction
- . deciding to communicate that metaphor
- . choosing context
- . choosing audience
- . selecting segments of the culture as signs (Pantopicality)
- . choosing the time of presentation
- . sequencing linearly
- . choosing evanescent or distanced form (multicanality)
- . choosing a direction for presentation of the flow of signs
- . choosing the order of signs (grammaticality)
- . sustaining the whole abstraction in memory until its
"utterance" is complete
- . acting as if he is offering something to be understood
and expects to be understood

Sign Selection Decisions - Mandatory

- . conventional to unconventional (Productivity)
- . "direct" reference to metaphoric (Pantopicality)
- . personal to impersonal
- . simple to complex
- . specific to general (Referentiality)
- . "beautiful" to "ugly"
- . perceptible to imperceptible (Displacement)

Sign Presentation Decisions - Mandatory

- . choosing pace, slow to fast
- . choosing number, few to numerous (Lengthlessness)
- . choosing size, small to large
- . choosing pattern, unpatterned to rhythmic
- . choosing style, smooth to abrasive

Sign Presentation Options (choice according to intent)

multimodality (statement vs command)

negation

proportionality

prevarication

jocosity

deuterism

soliloquism

interrogation

Some Distinctive Traits of Language as Artifact

*metaphoricity

*sequentiality

grammaticality

stratification

dichotomy

deuterism

multicanalicity

multimodality

productivity

pantopicality

lengthlessness

propositionality

displacement

*jocosity

prevarication

soliloquism

cenemicity

referentiality

digitality

negation

interrogation

temporality

*Additions by author.

If you have questions about any of these, I will be grateful for your guidance. I am anxious to develop examples which will be provocative enough so that others more capable than I will take up the study of Eidetics.

In his marvelous book, The Structure of Scientific Revolutions, Kuhn observes "A new paradigm gains status because it is more successful than its competitors in revealing questions for research or solving problems that the group of practitioners has come to recognize as acute. To be more successful is not, however, to be completely successful either with a single problem or notably successful with a large number."

I suggest to all of you that visual language and visual languaging are so like the verbal that the likenesses are embarrassing if not acute. The thoughts I have presented are meant to stimulate thought and research. I wish to close by quoting Kenneth Pike when he said, "It is impossible that a layman should not make blunders, some of them perhaps serious. I am under no illusion in this respect, but simply had to take the risk." Pike, Kenneth quoting Wilbur Urban, preface to the second edition.

Bibliography

- Bronowski, J., Science and Human Values, New York, Harper & Row (Perennial), 1972
- Debes, J., The Eyeful Power, Wash., D.C., International Visual Literacy Assn, 1975
- Eisenstein, S., The Film Sense, New York, Harcourt Brace & World, 1975
- Fries, C., The Structure of English, New York, Harcourt Brace & World, 1952
- Hsu, Francis L. K., The Study of Literate Civilizations, New York, Holt, Rinehart and Winston, 1969
- Key, Mary R., Paralanguage and Linguistics, Metuchen, N. J., Scarecrow Press, 1975
- Lennenberg, E., Biological Foundations of Language, New York, Wiley, 1967
- Makkai, A. and Lockwood, D., Readings in Stratificational Linguistics, University of Alabama, 1973
- Morris, C., Signification and Significance, Cambridge, MIT Press, 1964
- Pike, K., Language in Relation to a Unified Theory of the Structure of Human Behavior, The Hague, Mouton, 1967
- Stokoe, W., Semiotics and Human Sign Languages, The Hague, Mouton, 1972
- Wescott, R., Language Origins, Silver Spring, Md., Linstok Press, 1974
- Worth, Sol., Through Navaho Eyes, Bloomington Ind, Indiana U. 1975

A MONITOR SYSTEM FOR BILINGUAL LANGUAGE PROCESSING

Loraine K. Obler and Martin L. Albert

Department of Neurology
Boston V.A. Hospital and B.U. School of Medicine

Traditionally, neurologists studying polyglot aphasia have posited a "bilingual switch mechanism." This switch mechanism would be responsible for allowing the healthy bilingual to speak appropriately in one of her languages with little or no interference from the other. Neurologists deduced that such a switch must exist in the healthy bilingual on the basis of its apparent impairment in some cases of polyglot aphasia. The patient might be limited to speech in only one language (Bychowski, 1919), or she might switch languages inappropriately (Goldstein, 1948), or even mix languages within a sentence (L'hermitte et al., 1966). Because such a switch deals with language production, it has been referred to as an output switch.

Psychologists, Kolers (1966) and Macnamara (1971) in particular, have differentiated this output switch from an input switch. The input switch, they maintain, would be responsible for setting the language processing system to understand acoustic or written stimuli in one or the other of the bilingual's languages. Such a switch, these authors suggest, operates automatically, because it cannot be consciously controlled, nor experimentally manipulated (Kolers, 1965; Dalrymple-Alford, 1967). One does not decide to listen in a certain language.

The notion of a switch implies that the system(s) it controls must be either on or off. We may ask what it is that turns one language on or off, i.e. what triggers the switch. Macnamara has argued that factors in the environment (1967) or a "preattentive mechanism" (1971) must be responsible for setting the input switch. In this paper we propose that the responsible mechanism is a monitor system, which may operate continuously throughout language processing.

Before presentation of the experimental data, we wish to discuss two relevant phenomena of bilingualism which linguists have observed. Taylor (1976) has noted that at times the bilingual will fail for a few seconds to comprehend a language which he in fact knows, because he has not expected to hear that language. Apparently contradictory is the situation in which one is surprised to have understood an overheard comment because the language it was spoken in was totally unexpected. This contradiction can perhaps be resolved if we consider that in both instances the listener understood what she had heard after several instants. Si, par exemple, on commence à écrire en

français, au milieu d'un discours en anglais--if some French material interrupts in an English article, the reader may soon either catch the orthographic cues that English is no longer employed, or else note that the words make no sense in English, and she may try processing in another language. Im pitom tsrikim likro lo beanglit ele be'ivrit,*if one starts to read a perhaps unknown language, no immediate switch will recognize that the language is unknown. One will initially attempt to process the input in the languages one knows, and only later recognize and accept that the language is unknown.

Further evidence against a simple automatic input switch mechanism can be derived from a series of bilingual Stroop tests. In the original test, Stroop (1935) instructed subjects to label the ink colors of a card containing designs, and then of a card containing color-words whose ink-color conflicted with the color denoted by the word (e.g., red might be written in green ink). He reported that the time necessary to label the color-word cards was significantly longer (74%) than the time necessary to label the design card. Preston and Lambert (1969) have administered this test to several groups of bilinguals: French-English, German-English and Hungarian-English. In addition to labelling design cards in each of their languages, subjects were instructed in the same language condition to label in each language a card with color words in that language. In the cross language condition they were to label the same two color-word cards, but in the language opposite to that in which the words were written. We administered a similar test to Hebrew-English bilinguals in Jerusalem. In our experimental design we included non-balanced as well as balanced bilinguals, in order to clarify the interaction of Stroop interference and proficiency in a second language.

Subjects and Methodology

Our subjects were 48 college-age female and male speakers of both Hebrew and English. Sixteen of them were balanced bilinguals; they had learned both languages before the age of six, and evaluated themselves to control both languages equally well at the time of testing. Sixteen subjects were Hebrew-dominant speakers of English, and 16 were English-dominant speakers of Hebrew. We shall refer to these groups as the Hebrew and English groups respectively.

Subjects were instructed in one language to label, in that language, the ink colors of 100 designs as quickly as possible without making errors. These designs were of five colors: red, green, black, brown, blue, randomly ordered on the page. The time from the first label to the final label was measured, and note of

*If suddenly one must read, not in English but in Hebrew,

errors was made. Subjects were then given a Stroop card with 100 color words on it in either English or Hebrew. The individual color words conflicted with the ink colors they were written in. Subjects were again told to label the ink colors in the language they had been using for the designs. Then the card with color words in the remaining language was given. At this point, instructions were given in the second language and the design card and the two language cards were presented. English speakers responded first in English, then in Hebrew, while Hebrew speakers responded in Hebrew first. The order of language use was counter-balanced for the balanced group. The order of the two language cards was counterbalanced for each group. When labelling the design cards in English, subjects started at the upper left and proceeded to read down the columns. In labelling designs the Hebrew started at the upper right. Regardless of the language they were speaking, when labelling the word cards subjects were asked to start at the upper left when the card contained English words, and at the upper right when it contained Hebrew words.

Results

As Table I demonstrates, labelling the Hebrew stimuli took longer than labelling the English stimuli, for both the balanced bilinguals and the respective native speakers of these languages. In order to eliminate this language-specific effect (which may simply result from the fact that most of the Hebrew words were two-syllable words, while the English words were one-syllable words) we subtracted a constant of 6.4 seconds from all scores which involved labelling in Hebrew. A mixed design analysis of variance (two repeated measures: the nature of the stimuli and language of labelling, and one between subject variable: the bilingual groups) was performed on the corrected data. The third order interaction, language $\times$ condition $\times$ group, proved to be highly significant $p < .001$, so comparisons were made according to the Neuman-Keuls a posteriori two-tailed t-tests, which control for the number of comparisons made. Pertinent to our argument are the following results: there was no overall difference in response time across groups. Yet the balanced bilinguals tended to label designs in more time than did the respective monolinguals in their native language. For design labelling, both the dominant groups took longer in their second language than in their first ($p < .01$).

Both the same language and cross language conditions took longer than the design labelling conditions for all groups ($p < .001$). For the dominant groups, stimuli in the L2 provided less interference than stimuli in L1 ($p = .05$). The bilinguals were equally affected, in both cross-language conditions, and to a degree intermediate between the two degrees of interference experienced by the monolingual groups.

Table II presents the data on a form which permits us to compare it with Stroop's finding that, for his monolinguals, labelling 100 color words required 74% more time than had labelling 100 designs. Only our English speakers and balanced bilinguals, and only in the English-English condition, exhibited so much interference. The Hebrew speakers as a rule experienced less interference than the other two groups, especially in the L1L1 condition. Print in the first language elicited much less interference if one were labelling in the second as opposed to the first language. Print in the second language elicited the same amount of interference regardless of the language of labelling.

Errors proved to be similar to those reported by Preston and Lambert (1969). In the same language condition, subjects would read a word aloud instead of labelling the ink-color in which it was written. In the cross-language condition, subjects would never read the word aloud; rather, they pronounced its translation equivalent.

Discussion

It is evident that, as others have noted in similar tests, no simple on-off input switch is operating for our subjects. Even in the cross language condition, given two languages as orthographically and morphologically dissimilar as Hebrew and English, the subjects were unable to employ the optimal strategy of ignoring written stimuli. Nevertheless, if one was not proficient in a language, use of that language in this test, whether for labelling color designs or written stimuli, provoked less interference. Yet from the fact that in erring in the cross language conditions subjects spoke translation equivalents of the written words, we can conclude that they were processing the language stimuli of the input at a semantic level.

The error data further suggest that a more stringent control operates over the output speech than over the input; once a subject is set to speak a certain language, he will continue in that language.

In order to account for the data presented above, we propose a bilingual monitor system. The bilingual processing system may be seen as incorporation of a bilingual monitor feeding into an analysis-by-synthesis system (e.g. Fodor, Bever, and Garrett, 1974). In a first step in input processing, acoustic stimuli are assigned to phonemes. That the bilingual can perform this operation without regard for language-specific information is suggested by studies of phoneme perception (Caramazza et al., 1972; Garnes, 1976; Obler et al., forthcoming) which indicate that acoustic input is perceived by the bilingual in a fashion intermediate to that of the respective monolinguals. After phoneme assignment, an attempt to match phoneme strings to

lexical items must be made. Since it would be inefficient for the matching element to scan through the entire lexicon, Fodor et al. have argued that some methods of prediction are employed. The matching element can then test-match high probability lexical items against the phoneme strings it has identified. If a predicted match fails to materialize or to make sense in the light of following context, other lexical items must be searched. The bilingual monitor system we propose would participate in the prediction procedure. The monitor would scan incoming data for cues that one or the other language was being spoken, and then channel the matching element towards lexical items in that language. The linguistic cues which the monitor could take into account might be specific phonemes or consonant clusters which obtain in only one of the two languages. It might identify a word or syntactic structure which was peculiar to only one of the two languages. It would take into account that a certain sentence or conversation had made sense in one language, and register the probability that that language would continue being spoken. The monitor could also incorporate non-linguistic, environmental information into its predictive decisions.

Although the matching element might be primed to check items from a specific language first, it would always be prepared to process items in the other language, and might even attempt to process both languages concurrently. In essence then, one would not have an on-off input switch, but rather a flexible, continuously operating monitor system, sensitive to changes in the linguistic and non-linguistic environment. The monitor will never entirely switch one language off; it will channel efforts, priming the language processing systems by constructing hierarchies which predict the probability of occurrence of items or sets of items, in order to enable the bilingual listener to process language efficiently.

Bibliography

- Bychowski, Z. 1919. Ueber die Restitution der nach einem Schaedelschuss verlorgangenen Sprachen bei einem polyglotten. Monatschrift fur Psychiatric und Neurologie, Bd. XLV. Heft 4, 45, 183-201.
- Dalrymple-Alford, E. 1967. Prestimulus language cueing and speed of identifying Arabic and English words. Psychological Reports, 21, 27-28.
- Fodor, J., Bever, T., and Garrett, M. 1974. The Psychology of Language, New York: McGraw Hill Book Company.
- Garnes, S. 1977. Effects of bilingualism on perception in Papers in Psycholinguistics and Sociolinguistics, ed. O. K. Garnica, Columbus, Ohio: Department of Linguistics, Ohio State Univ., Feb. 1977, 1-10.
- Goldstein, K. 1948. Language and Language Disturbances. New York: Grune and Stratton.
- Hamers, J. and Lambert, W. 1972. Bilingual interdependencies in auditory perception. J. of Verbal Learning and Verbal Behavior, II, 303-310.
- Kolers, P. 1965. Bilingualism and bicodality. Language and Speech, 8, 122-126.
- Kolers, P. 1966. Reading and talking bilingually. American J. of Psychology, 79, 357-376.
- L'hermitte, R., Hecaen, H., Dubois, J., Culioli, A., and Tabouret-Keller, A. 1966. Le probleme de l'aphasie des polyglottes: remarques sur quelques observations. Neuropsychologia, 4, 315-329.
- Macnamara, J. 1967. The bilingual's linguistic performance: a psychological overview. J. Social Issues, 23, 58-77.
- Macnamara, J. and Kushnir, S. 1971. Linguistic independence of bilinguals: the input switch. J. of Verbal Learning and Verbal Behavior, 10, 480-487.
- Preston, M. and Lambert, W. 1969. Interlingual interference in a bilingual version of the Stroop color-word task. JVLVB, 8, 295-301.
- Stroop, J. 1935. Studies of interference in serial verbal reactions. J. of Experimental Psychology, 18, 643-662.
- Taylor, I. 1976. Introduction to Psycholinguistics. New York: Holt, Rinehart and Winston.

Acknowledgements

Thanks are due to Peter Salaman and Jerry Levinson for assistance in collecting the Stroop data. For statistical analyses and advice about manipulation of the data we thank Errol Baker.

TABLE I: Average Response Times (seconds)

Stimuli	Subjects:					
	Balanced Bilinguals N = 16		English Dominant N = 16		Hebrew Dominant N = 16	
	Hebrew Labelling	English Labelling	Hebrew Labelling	English Labelling	Hebrew Labelling	English Labelling
Designs	A 63.4	B 56.4	C 73.8	D 51.2	E 56.9	F 67.5
English Print	G 89.4	H 96.8	J 102.8	K 94.8	L 74.7	M 91.9
Hebrew Print	N 97.7	P 82.4	Q 105.3	R 73.8	S 82.9	T 88.2

Table II: Percent of Time Increment over Labelling Designs

Subjects:

Conditions (print, label)	Balanced Bilinguals	English Dominant	Hebrew Dominant
Same-language L ₁ L ₁ L ₂ L ₂	72% (English) 54% (Heb.)	85% 43%	46% 36%
Cross-language L ₁ L ₁ L ₂ L ₂	41% (E,H) 46% (H,E)	39% 44%	30% 31%

CEREBRAL LATERALIZATION AND PLASTICITY:
THEIR RELEVANCE TO LANGUAGE ACQUISITION

Marc L. Schnitzer

La Universidad de Puerto Rico

One of the principal sources of interest in the question of cerebral lateralization for language is the fact that children acquire second languages to a degree indistinguishable from native competence, whereas adults do not. At the very least, adults retain some degree of a foreign accent regardless of how well they learn the post-pubescently acquired language.

As noted by Scovel (1969), there have been attempts to explain this situation on the basis of differences in the language-learning situation between children and adults. Thus Wolfe (1967) argues that the fact that children learn second languages unconsciously, whereas adults learn them consciously may account for the difference in second-language performance between these groups. What Wolfe cannot explain is just why these two groups should use these different strategies in learning second languages; he merely displaces the question, rather than answering it. Newmark and Reibel (1968) claim that it is the difference in situation under which children and adults acquire second languages (being natural, and artificially structured, respectively) which accounts for the difference. This does not explain the fact that adults thrown into a foreign speech community are not any better at second-language acquisition than their adult counterparts in the foreign language classrooms. Geschwind (1966) believes the difference may be due to the fact that children use visuo-auditory and tactuo-auditory associations in learning a language, whereas adults learn languages by translating from an already-acquired language, and therefore use auditory-auditory associations. Scovel questions the relevance of Geschwind's "explanation" since children acquiring a second language in a classroom situation (and who are therefore forced to use auditory-auditory associations) still demonstrate the superiority over adult learners. He also questions why, if Geschwind is correct, this superiority should be manifested primarily in phonology.

Scovel presents an alternative to these "nurturist" proposals in which he suggests that it is lateralization of cerebral function for language which may be the cause of this difference. He claims that the prelateralized cortex can handle the task of language acquisition in a way in which the lateralized one cannot. He cites Penfield's (1965) observation that children under ten or twelve years of age can lose the ability to speak when the left hemisphere speech areas have been destroyed. Penfield notes that he has seen such children recover after a year of "dumbness and aphasia", speaking as well as ever "because the young child's

brain is functionally flexible for the start of a language. They start all over again" (786). But Penfield also notes that after this age, the functional connections for speech have been fixed and that subsequent cortical damage will not allow the speech areas to be transferred to the contralateral cortex. "This 'non-dominant' area that might have been used for speech is now occupied with the business of perception" (Penfield, 1965, 792, cited by Scovel, 1969, 251).

Lenneberg (1967) presents a table summarizing developmental stages of language. This table indicates that below the age of twenty months, when the child is at the babbling stage, symptoms and prognoses after damage to either hemisphere are the same. From 21 to 36 months, language acquisition occurs and hand preference develops. From age three to ten years, some grammatical refinement takes place. Lenneberg states that cerebral dominance is established between ages three and five, but notes that the right hemisphere may still be involved in speech and language functions (with "about 1/4 of early childhood aphasia due to right hemisphere lesions" (180)). From ages eleven to fourteen, foreign accents emerge, and from the middle teen years on, acquisition of second languages becomes more and more difficult. At this point, approximately 97% of the population has language clearly lateralized to the left (Lenneberg, 1967, 180-1).

If Lenneberg's chart is correct, then at least correlational evidence is provided for Scovel's thesis. What, then, is the evidence for Lenneberg's conclusions? Lenneberg's principal source is Basser (1962), a study of 102 cases of hemiplegia of early onset in which "the lesion was in the left hemisphere in 48 cases and in the right in 54. Hemispherectomy was performed in 35 cases" (Basser, 1962, 428). Basser notes three patterns of development among the hemispherectomized patients:

1. Both hemispheres participate in language development prior to lateralization to the left hemisphere. Bassere discusses three cases of right-handed children with right hemisphere lesions which caused speech disturbance, one of which is especially interesting because the brain damage took place at age five and clearly the lateralization to the left hemisphere had not yet occurred.
2. Left hemispheric lateralization occurs early. Basser notes two such cases.
3. Right hemispheric lateralization occurs early. Basser notes two of these cases and states that "the relative frequency of these modes of development awaits further study but it would appear that the first is frequent and certainly the third is rare" (Basser, 1962, 450).

Lenneberg concludes from Basser's data that the consequences of a left hemispherectomy depend on the age at which the brain damage first occurs. Early lesions would cause language development to take place in the hemisphere contralateral to lesion, so

that the later removal of the lesioned hemisphere would not cause aphasia (Lenneberg, 1967, 152). He concludes that at first both hemispheres are involved in language development but that the right hemisphere becomes gradually less involved unless the left hemisphere is damaged and hence incapable of assuming the role of language function (Lenneberg, 1967, 151).

But as Krashen (1973) notes, Bassier's data are consistent with the view that the completion of lateralization occurs by age five, rather than be age eleven as indicated by Lenneberg's chart cited above, since in all cases of right hemisphere damage leading to speech problems occurred before age five. Bassier's case 15 stands out as the sole example of a right lesion incurred after age five (It occurred at age ten.), and there was no language disturbance in this case. Hence, Lenneberg's position is not supported.

In this paper, Krashen cites evidence from dichotic-listening experiments that lateralization is probably complete by age four. The hypothesis that lateralization is not complete until puberty would predict increasing right-ear superiority with age throughout childhood on dichotically presented language tasks. But Harshman and Krashen (1972) found "no significant change in degree of lateralization or right ear advantage . . . for the age range tested, from four to nine" (Krashen, 1973, 66). And the degree of lateralization found in these children was the same as that found for adults under similar testing conditions. Krashen even goes so far as to imply, on the basis of Carol Chomsky's (1969) discovery that five year olds do not have full linguistic competence, that lateralization may be something necessary to achieve the completion of language acquisition.

Thus far, all the investigations cited have agreed that lateralization is a process which takes place during childhood. They have disagreed as to when it is completed and as to its relevance to language acquisition. But Molfese (1972), and Molfese, Freeman, and Palermo (1975) present evidence that language is lateralized from birth. They tested three groups consisting of ten adults aged 23 to 29, of eleven children aged 4 to 11, and ten infants aged one week to ten months. It was found that when speech stimuli and non-speech auditory stimuli were presented, there were no significant differences among the groups, with respect to auditory-evoked-potential measurements, taken with electrodes placed on both hemispheres. Most subjects in all three groups showed a significantly greater left-hemisphere auditory-evoked-potential (AEP) response for speech sounds and greater right-hemisphere AEP responses to mechanical non-speech sounds (a C major piano chord and a burst of synthetic noise). No subject responded to all stimuli equally in both hemispheres, although one did respond to nearly all stimuli with more activity in the right hemisphere. A one-week-old infant showed responses that were left lateralized for verbal stimuli and right lateralized for mechanical stimuli. Thus

Molfese concludes that "lateralization of function occurs prior to one year of age and, on the basis of Subject 38's responses, may be present even at or prior to birth" (Molfese, 1972, 44).

In addition to these behavioral data, there is also some neuroanatomical evidence for prenatal hemispheric asymmetry. Geschwind and Levitsky (1968) reported the presence of a larger left temporal planum (the area behind Heschl's Gyrus) than right temporal planum in 100 adult brains. This raised the question of whether the asymmetry precedes or follows the acquisition of language. But recently, Wada, Clarke, and Hamm (1975) have found evidence of morphological asymmetry of both the temporal planum and the frontal operculum (adjacent to and sometimes considered to be part of Broca's Area) in the twenty-ninth week of gestation. The left was found to be larger than the right in approximately 90% of both the adults and infants (or fetuses) examined. These data are consistent with those of Tetzner, et.al. (1972), in which anatomic asymmetry was found in stillborn infants.

These data present a puzzle: if language is lateralized from before birth, why is it that adult hemispherectomies result in dysphasia, whereas virtually none of the childhood hemispherectomies do (Basser, 1962)? The answer may lie in some remarks made by Krashen (1973), in which he discusses the notion of transfer, "the ability of the minor hemisphere to take over the language functions in case of serious injury to the dominant hemisphere" (67). He criticizes what he takes to be Lenneberg's and Scovel's notion of transfer in which it is linked to lateralization as the ability of the right hemisphere to mobilize "those linguistic capacities of the right hemisphere which have not fully lateralized to the left hemisphere" (67) so that once lateralization is complete, transfer cannot take place. His criticism is based on the inference that if lateralization is complete by puberty, then transfer ought to be possible until puberty. However, he concludes that in fact perfect transfer is definitely possible before age five, rather than up to puberty, based on data such as Basser's, referred to above.

But Krashen is wrong. Perfect transfer is not possible before age five. The work of Maureen Dennis and her co-workers (Dennis and Kohn, 1975, and Dennis and Whitaker, 1976) on the acquisition of language by hemispherectomized (or at least hemidecorticated) children whose operations occurred before they started to acquire language (before five months of age) indicates that among children from whom the language function is taken over by the right hemisphere, language acquisition is inferior to that of people using the left hemisphere.

Dennis and Kohn (1975) tested the comprehension of syntax of four left infantile hemiplegics who subsequently underwent right hemidecortication and five right hemiplegics from infancy who subsequently underwent left hemidecortication. The groups

were of similar cognitive ability, no subjects showed clinical signs of aphasia, and none showed "the more subtle type of linguistic disturbance measured by the Token Test" (473). Subjects were asked to listen to active-affirmative, passive-affirmative, active-negative, and passive-negative sentences, and were asked immediately thereafter to select from a display of two pictures, the one which was correctly described by each sentence. It was found that, regardless of sentence, right hemidecorticates made more correct responses at shorter latencies than did left hemidecorticates. Also it was found that the left hemidecorticates made more errors in relating passive negative sentences to the appropriate picture.

Dennis and Whitaker (1976) performed detailed linguistic tests on three 9 to 10 year olds, all of whom had had hemispherectomies performed between the ages of 28 days and 4.5 months. There were two left hemidecorticates and one right hemidecorticate. Two types of test were used--(a) thirteen standardized tests of speech and language, the results of which could be compared with established norms, and (b) eight tests devised for this investigation or "adapted for children from adult neuro-linguistic tests" (408). On the basis of the results of these tests, the authors conclude that the right hemisphere is deficient at the following skills:

1. "Understanding auditory language, especially when meaning is conveyed by syntactic diversity" (428).
 2. Detecting and correcting errors in surface structure.
 3. Repeating stylistic variations of sentences.
 4. Forming tag questions from presented statements.
 5. Determining sentence implications.
 6. Combining syntactic and semantic information for replacement of missing pronouns.
 7. Judging the relationships among words in sentences.
- They conclude that the right hemisphere's relative deficiency in language processing is "an organizational, analytical, syntactic, and hierarchic problem rather than a difficulty with the conceptual or semantic aspects of language" (428).

On the basis of these studies, one must conclude that the left hemisphere is endowed from birth with a greater facility for learning certain crucial aspects of natural language than is the right hemisphere. Although up to some point of development, the brain is "plastic" and "transfer" can take place, so that language can be learned by the right hemisphere if necessary, it is not learned as well as it is by intact left hemispheres.

Thus we must be careful to be clear about what we mean by the notions lateralization and plasticity. Although the brain is lateralized for language from before birth, it is relatively "plastic" for the first five years of life, so that language lateralization can be "transferred" to the contralateral hemisphere in the event of neuropathology during this period. But the

evidence suggests that the contralateral hemisphere is not as capable of language acquisition as the left hemisphere would have been had it not been subject to neuropathology. Thus the brain is never completely "plastic".

How, then, do we explain the fact cited by Scovel (1969) that prepubescent individuals acquire second languages without a foreign accent, whereas adults do not? If lateralization is present from birth, and if the period of relative plasticity ends by age five, clearly this fact cannot be related to either lateralization or plasticity. Perhaps it may be related to other maturational developments. It is interesting to note that Dennis and Whitaker (1976) found no evidence of articulation deficits or phonological discrimination problems among any of the hemispherectomized subjects. Apparently, phonology can be mastered equally well by either hemisphere. Since phonology involves, *inter alia*, dividing a continuum into discrete chunks, it would be interesting to see whether the age at which foreign accents appear (in conjunction with second-language acquisition) correlates with the age at which other classificatory matrices become calcified--for example, at what age do musicians trained in Western music lose their ability to perceive exotic musical scales except in terms approximate to the Western diatonic scale? (I have elsewhere argued (Schnitzer, 1976) that phonology may not be as intimately embedded in linguistic structure as other aspects of language.) And in those rare cases of adults who retain the ability to acquire foreign-accent-free second languages, do these individuals also retain the ability to learn other (non-linguistic) classificatory chunkings beyond the age at which others lose this ability?

These questions require empirical investigation. Ultimately they should allow us to look at the complex interrelations involved in the brain's acquisition of language, in a way that can help us to understand the whole in terms of the parts.

REFERENCES

- Basser, L. 1962. "Hemiplegia of Early Onset and the Faculty of Speech with Special Reference to the Effects of Hemispherectomy". Brain 85. 427-60.
- Chomsky, C. 1969. The Acquisition of Syntax in Children from 5 to 10. Cambridge, Massachusetts: M.I.T. Press.
- Dennis, M. and B. Kohn. 1975. "Comprehension of Syntax in Infantile Hemiplegics after Cerebral Hemidecortication: Left Hemisphere Superiority". Brain and Language 2. 472-82.
- _____, and H. Whitaker. 1976. "Language Acquisition following Hemidecortication: Linguistic Superiority of the Left over

the Right Hemisphere". Brain and Language 3. 404-33.

Geschwind, N. 1966. "A Discussion of 'Speech Development: Its Anatomical and Physiological Concomitants' by Eric H. Lenneberg". Brain Function: Volume III. ed. by Carterette, 61. Berkeley: The University of California Press.

_____, and W. Levitsky. 1968. "Left/Right Asymmetries in Temporal Speech Region". Science 161. 186-7.

Harshman, R. and S. Krashen. 1972. "An 'Unbiased' Procedure for Comparing Degree of Lateralization of Dichotically Presented Stimuli". U.C.L.A. Working Papers in Phonetics 23. 1-13.

Krashen, S. 1973. "Lateralization, Language Learning, and the Critical Period: Some New Evidence". Language Learning 23. 63-74.

Lenneberg, E. 1967. Biological Foundations of Language. New York: Wiley.

Molfese, D. 1972. Cerebral Asymmetry in Infants, Children, and Adults: Auditory Evoked Responses to Speech and Noise Stimuli. Ph.D. dissertation. University Park, Pennsylvania: The Pennsylvania State University.

_____, R. Freeman, and D. Palermo. 1975. "The Ontogeny of Brain Lateralization for Speech and Nonspeech Stimuli". Brain and Language 2. 356-68.

Newmark, L. and D. Reibel. 1968. "Necessity and Sufficiency in Language Learning". I.R.A.L. 6. 145-64.

Penfield, W. 1965. "Conditioning the Uncommitted Cortex for Language Learning". Brain 88. 787-98.

Schnitzer, M. 1976. "The Role of Phonology in Linguistic Communication: Some Neurolinguistic Considerations". Studies in Neurolinguistics, Volume I. ed. by Whitaker and Whitaker. New York: Academic Press.

Scovel, T. 1969. "Foreign Accents, Language Acquisition, and Cerebral Dominance". Language Learning 19. 245-53.

Teszner, D., A. Tzavaras, J. Gruner, and H. Hécaen. 1972. "L'asymétrie droite-gauche du planum temporale: a propos de l'étude anatomique de 100 cerveaux". Revue Neurologique 126. 444-449.

Wada, J., R. Clarke, and A. Hamm. 1975. "Cerebral Hemispheric Asymmetry in Humans". Archives of Neurology 32. 239-46.

LANGUAGE IMPAIRMENT AND RECOVERY IN POLYGLOT APHASIA
A CASE STUDY OF A HEPTA-LINGUAL

Linda Galloway
Department of Linguistics
University of California at Los Angeles

I want to talk today about some aspects of polyglot aphasia. Since some people attending this conference might be unfamiliar with the subject matter, I would like to make some preliminary comments. First of all, aphasia is a linguistic disturbance associated, for most individuals, with damage to a more or less circumscribed area of the left cerebral hemisphere of the brain.

Secondly it is unclear at this point in time not only what factors contribute most, if any do, to language recovery and language impairment in polyglot aphasia, but also as to what the general patterns of recovery and impairment are. For example, some case histories report that the languages of the polyglot recover simultaneously, some that one language emerges only after the other language has recovered, and still others cite cases where a second language is either recovered while the first regresses or is never recovered at all. (Paradis, in press.)

Several factors may contribute to recovery pattern, e.g. the locus, extent and severity of lesion, learning contexts as well as age, order and modality of acquisition, degree of premorbid fluency, the language of the hospital staff or the language trained in speech therapy, premorbid frequency of use, social and affective variables, and cerebral dominance and processing styles.

Unfortunately the case histories, upon which theories of recovery are based and which range back in time over 100 years, are often incomplete and lacking in uniform test instruments across all languages, or they are selective in what is reported. Paradis (in press) cites one example where the patient was neither aphasic nor polyglot. The cases in the older literature were described in a linguistic vacuum at a point in time before linguistics had developed in this century and become the science it is today.

In my paper today I will discuss the case of MB who during the time before his stroke had had close contact with seven different languages. First I will present his language and medical histories, then I will discuss his aphasia and report the results of a comprehension task performed across all languages. As it would be impossible to address all theories of bilingual aphasia, I will discuss two in light of the case of MB. The first is Ribot's theory that suggests language recovery replicates order of acquisition; the second is Pitres' hypothesis that proposes recovery pattern is determined by the frequency of language use prior to cerebral insult. Finally I will raise questions and implications.

In Table 1 is MB's language history. MB was born in 1926 into a prosperous, Jewish business family in Hungary. The first acquired language was, therefore, Hungarian which was the language both of his family and community. When MB was four, the family moved to Poland for two years. During this time he became fluent in Polish and, it appears, to the exclusion of his native Hungarian. His primary caretaker was a Polish governess, he attended a Polish nursery school, and although his parents continued to speak Hungarian with each other, his mother was forced to learn minimal Polish to communicate with him. MB did not use Polish again after this time. When he was six, the family returned to Hungary. MB reports relearning Hungarian at this time and was held back in school as a result.

When MB was ten, the family moved again and this time to Rumania to a multi-lingual neighborhood and town whose four language groups were Rumanian, Hungarian, Yiddish and German. The family lived in Rumania for two years and MB was sent to a Rumanian school. Thus he had to learn to read, write and speak some Rumanian. The language was also spoken socially. In the afternoons he attended Hebrew school where the only lingua franca for instruction in this multi-lingual town was Yiddish. MB learned Yiddish and also used the language socially outside the classroom. This two year span represents the frequency peak in the usage of Yiddish.

From the age of 12-17 MB lived in Hungary where Hungarian was again his primary language. He received some formal instruction in English and German in high school during this period, but had no opportunity to use these languages outside the classroom.

Concurrent with the language history to this point was his contact with biblical Hebrew. From the age of 3-17 he was sent to an Orthodox Hebrew school for 4-5 hours daily for instruction in this biblical language and religious texts.

MB's first need to use German was during the year spent in an Austrian concentration camp. After the war he went to Germany for six years, and German became his main language almost exclusively. He attended language instruction for foreigners, lived in a German neighborhood, enrolled in the university and received a degree in engineering.

At the age of 25 he immigrated to the United States, settled in an English speaking town, and without formal instruction concentrated on learning English from the newspapers, radio, at work and socially. For the past 25 years he has lived in the U.S. and English appears to be his primary and preferred premorbid language. Although he has married a Hungarian woman, English seems to be the predominant language in the home. He reports on occasion having used German and Yiddish but not often. He has infrequently attended Temple since his incarceration in the concentration camp 32 years ago.

Now, I will present MB's medical history. Handedness appears to be a factor in aphasia because of the observed correlation between left-handedness and superior language recovery. MB is ambidextrous to left-handed. The only task performed exclusively with

the right hand is writing which began as a result of corporal punishment on his first day of school.

At the age of 47 about three years ago MB suffered a massive left-sided CVA and was comatose for 21 days. Surgery performed shortly after admission to the UCLA medical center revealed that the left posterior temporal, parietal and opercular regions were edematous with probably secondary vascular occlusion. Findings were negative for intracerebral hematoma. An EMI-scan indicates a large cystic space in the left parietal lobe. Upon regaining consciousness, MB displayed a severe expressive aphasia with some receptive disturbances.

MB's current language abilities are displayed in Table 2. With respect to expressive abilities MB's speech in English is fluent, syntactically complex and marked with some word substitutions and word finding problems. His speech production in the other six languages is at best limited to the occasional recall of isolated words. Regarding receptive abilities MB displayed no comprehension deficits in English on the Schuell Aphasia Test nor in my interviews with him. Observationally he also showed no comprehension problems in German. In lengthy discussions free of contextual or nonverbal cues I spoke German (in which I possess near native competence), and he responded in English. Finally MB reported he could understand Hungarian and Yiddish but felt he would not be able to understand Hebrew, Rumanian or Polish.

Because aphasic test batteries were not available in all of the languages, or the persons to administer them, the following rough measure of auditory comprehension was devised. Texts of about 60-70 words were selected for each language, and the samples were recorded on audio tape by different individuals who were native speakers, except for the German which I recorded. The Hebrew text, a lesser known biblical narrative from Kings, was read with the Ashkenazi pronunciation of Eastern Europe; the Yiddish passage was read with 'Standard Yiddish' pronunciation free of regional traces. The Polish text differed from the rest in that it was linguistically simpler and consisted of samples considered appropriate for a child.

MB was told which language he was to hear and was given a general reference to the content, e.g. "Now I'm going to play a short passage in Hungarian about the paprika industry in Hungary." He was instructed to retell, if possible, the sample in English or report anything he understood.

The graph contains the results. In Polish MB was unable to identify single lexical items with any certainty. From the Rumanian and Hebrew samples he reported a few individual words or phrases. In the other four languages he retold 3/4 or more of the content fluently and accurately.

I will now discuss two theories of language recovery in polyglot aphasia in light of the evidence from the case of MB. The first hypothesis is that of Ribot (1906) who suggested that the earliest acquired language(s) would be most resistant to aphasic impairment and that languages acquired later would be most impaired. This hypothesis predicts that language recovery will

follow the order of acquisition.

In Table 3 in the left-most column, MB's languages are listed in order of acquisition. Ribot's hypothesis predicts a range of impairment and recovery pattern from Hungarian, which should be least impaired and the first language to recover, to English, which should be most impaired and last to recover. In the next column MB's observed language recovery and the impairment suggested by the comprehension task is listed on a scale ranging from normal expressive/receptive abilities down through decreasing degrees of comprehension. There appears to be no strong correlation between order of acquisition, on the one hand, and impairment and recovery on the other. In particular, the position of English is at the opposite end of the predicted scale. It was the language first to recover and is the least impaired. Further, Polish is at the bottom of the observed impairment scale and displayed the lowest ranking on the comprehension task. It was, however, one of the very earliest acquired languages--spoken from age 4 to 6. According to Ribot's hypothesis one would expect Polish to be among the least impaired. In addition one would expect Rumanian which was acquired at age ten to be less impaired than German which was not actively spoken until ten years later. In contrast the observed results are the reverse. Similarly German should be more impaired than Yiddish, but both reflected comparable ratings on the task. In summary: the observed recovery pattern and the findings from the auditory comprehension task of this subject do not lend support for a theory of recovery based on acquisition order.

The second theory of polyglot aphasia which I plan to discuss today is that of Pitres (1895) who hypothesized that the languages most frequently and extensively used prior to cerebral insult would recover first. In general this is the case with MB; English was the primary language before the stroke and recovered first. More specifically two possible measures of practice effects are frequency and recency of use at the point in time immediately prior to the stroke. In Table 4 the left column is frequency measured in the absolute number of years (in parentheses) that MB's languages were primary or were most actively used. The list is in decreasing order of frequency. The right hand column is a recency ranking in decreasing order; the measure is the number of years since the languages had last been most frequently used prior to the stroke.

More specifically Pitres recovery pattern proposes that after the patient has regained the ability to understand and then to speak the language which was most familiar to him, one would observe the gradual return of the ability to understand the other languages which the patient knew. Although Pitres does not specifically say so, one possible extrapolation of his recovery pattern which is logically consistent with such a frequency hypothesis and, further, which is plausible in the case of a multilingual aphasic, would be first the return of the ability to understand the other languages less frequently used, followed by the possible return in comprehension of the languages least frequently

used before brain damage. (I shall refer to this as the amended Pitres recovery pattern.)

In Table 4 the languages fall roughly into three frequency groups. English was the language used most frequently and recently before the stroke. Yiddish, Rumanian and Polish were used least and are most remote in time with Hungarian, Hebrew and German falling roughly in between these two extremes.

In Table 5 the amended recovery pattern is listed along with the recovery pattern expected from the three frequency groups from Table 4, and the observed recovery pattern. One can see some correlation between the expected and observed recovery pattern. The results are at variance in two languages, Yiddish and Hebrew. First, Yiddish. It ranks low on the expected comprehension scale, because the only period which it was used with the greatest frequency was for the short two year span in Rumania 35 years before cerebral insult. At this time both Rumanian and Yiddish were acquired simultaneously at the age of ten, both were used daily as a language of instruction as well as socially outside the classroom, and both were spoken for the same length of time from the age of 10 to 12. Strictly on the basis of a frequency/recency measure one would expect comparable impairment in the two languages which is not the case.

On the comprehension task, Yiddish ranked highest along with German, English and Hungarian. It is unlikely that brain damage would enhance MB's proficiency in Yiddish and thus, in effect, raise his performance in Yiddish up to a comprehension par with that of the other three languages. It is quite possible, however, that his continued, even though minimal contact with Yiddish up to the time of the stroke helped to maintain comprehension of the language. Further it is plausible that the performance on the comprehension task reflects his fluency in Yiddish before the stroke.

In Table 6 MB was asked to rate his premorbid comprehension on a seven point scale ranging from zero, 'None/Almost none,' to 7, 'Native-like,' for all the languages. The four highest ratings were assigned to the four languages which also displayed the highest performance scores on the comprehension task. In other words, it would seem that MB could understand Yiddish as well as the other three languages prior to the stroke.

A second possible factor contributing to the high performance in Yiddish is its close relationship to German. Of the seven premorbid languages German and Yiddish are not only historically closely related, but the two grammars also share a high degree of linguistic similarity. In fact, 'Standard Yiddish,' that is a Yiddish free of regional or dialect variations which is what MB heard on the task, and German are to a high degree mutually intelligible. This case points to the possibility that the degree of linguistic similarity in the premorbid grammars of the polyglot aphasic could be a variable involved in language recovery.

The second language whose performance score was **other** than predicted by a frequency hypothesis was Hebrew. It differs from all the other premorbid languages, however, first in its usage.

The Hebrew of the Old Testament and liturgy which MB studied is no longer spoken today; it is a dead language. Thus it was not actively used like the other languages in serving daily communicative needs, but was restricted to classroom study and religious ceremony. Secondly, it was acquired primarily in the visual modality. Given these facts it is not surprising that one would score lower in such a language (even without brain damage) on an auditory comprehension task. MB's comprehension of Hebrew, however, does not seem completely impaired. At an observational level he was able to translate quite quickly and correctly about 2/3 of the test sample into English from the printed text. Unfortunately results of systematic testing in the visual modalities across all premorbid languages are not available for comparison. The case does point to the possible influence already discussed by others (Hinschelwood, 1902; Critchley, 1974; Galloway, 1976; Wechsler, 1977; Pardis, in press) of both environment of usage and written vs. oral acquisition on recovery in polyglot aphasia.

In conclusion, the case of a hepta-lingual aphasic and the results of an auditory comprehension task were presented. The findings did not provide evidence in support of a theory of recovery based on order of acquisition. Frequency, however, appeared to be an important variable in language recovery, but it did not account for the observed data completely. Language-Relatedness and premorbid fluency in addition to modality of acquisition and context of usage appeared to be other possible variables involved in impairment.

The case presented today is illustrative of some of the issues in polyglot aphasia, and the findings are meant to be suggestive only. There are compounding factors. For example, MB is not representative of an aphasic population due to his left-handedness and, thus, the increased probability of bilateral representation of language in the brain. In fact the excellent recovery of English in view of the severity of the cerebral trauma indicates just such a possibility. Secondly the nature of the comprehension task is not without artifacts. The ability to paraphrase and to translate from one language to another could involve mechanisms other than those needed for the immediate processing of language. More rigorous testing designed to tap more directly all levels of the subject's grammars must be conducted.

Finally, why is the kind of research discussed today of scientific interest? How can linguistics make a specific contribution? Controlled, experimental studies of bilingual or polyglot aphasia are in a unique position with respect to studies in monolingual aphasia to contribute to our understanding of Language and languages in the brain. Are there, e.g. substantial differences between bilingualism and polyglotism with respect to functional organization? Are there universal aspects of language processing? Are all languages stored in the same manner and subserved by the same underlying mechanism, or are they functionally separate? What can bilingual aphasia tell us about the possible differences in first and second language acquisition and the influence of age, modality and manner of acquisition? Research in

bilingual aphasia potentially offers a contribution to the growing body of evidence on cerebral dominance, possible hemispheric maturational differences and the lateralization of language and critical age, as well as implications perhaps for such areas as clinical treatment and second language teaching.

.....

Table 1: Language History

<u>Age</u>	<u>Country of Residence</u>	<u>Language</u>	<u>Environment of Acquisition</u>
0-4	Hungary	Hungarian	Family
4-6	Poland	Polish	Polish governess Polish nursery school
6-10	Hungary	Hungarian	'Relearn' Home, school, friends
10-12	Rumania (multilingual town)	Rumanian	Language of instruction in school Friends
		Yiddish	<u>lingua franca</u> in Hebrew School Socially outside class
		Hungarian	Home
12-17	Hungary	German/ English	Some formal instruction in school
		Hungarian	Home, friends, school
3-17	(concurrent)	Hebrew (Biblical)	Orthodox Hebrew School, daily 4-5 hours Study of language & religious texts
18-19	Austria	German	Concentration Camp
19-25	Germany	German	Language instruction for foreigners German neighborhood University education, degree in engineering
25-present	U.S.A.	English	No formal instruction Learned from newspaper, radio, at work, socially

Table 2: Current Language Abilities

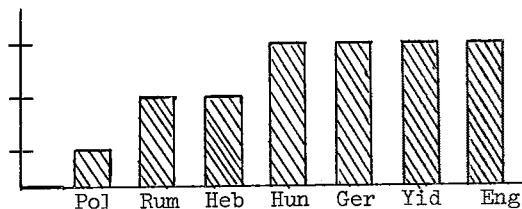
	Engl.	Ger.	Hung.	Yidd.	Heb.	Rum.	Pol.
Expressive Ability	Near Normal	None					
Receptive Ability (auditory)	Normal				Almost None		

Graph: Auditory Comprehension TaskParaphrase Ability

3/4 or more of content

Isolated words or phrases

None

Premorbid LanguagesTable 3: Ribot's Hypothesis

<u>Acquisition Order</u>	<u>Ribot</u>	<u>Observed Recovery & Impairment</u>	
1. Hungarian	least impaired recover first	English	normal expression & comprehension
2. Hebrew	↓	Hung/Germ/Yid	good comprehension
3. Polish			
4. Rumanian/Yiddish			
5. German		Rumanian/Hebrew	poor comprehension
6. English	most impaired recover last	Polish	no comprehension

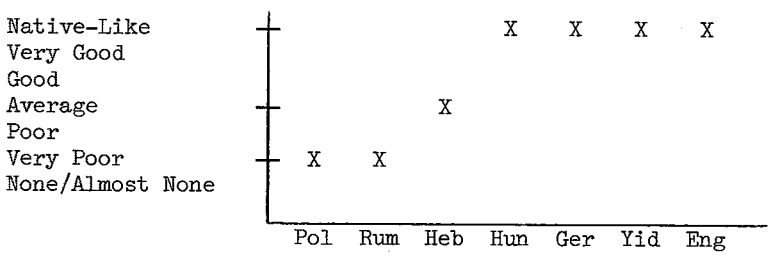
Table 4: Practice Effects Just Prior to Stroke

	FREQUENCY (in years)	RECENCY (in years)
I	English (22)	English (ø)
II	Hungarian (15) Hebrew (14) German (7)	German (22) Hebrew } (30) Hungarian }
III	Yiddish } (2) Rumanian } Polish }	Yiddish } (35) Rumanian } Polish (41)

Table 5: Frequency Hypothesis

	Amended Pitres Recovery Pattern	Expected	Observed
I	speak/understand	English	English
II	good comprehension	German Hungarian Hebrew	German Hungarian Yiddish
III	poor comprehension	Yiddish Rumanian Polish	Hebrew Rumanian Polish

Table 6: MB's (premorbid) Comprehension Rating



Acknowledgments

I want to express my deepest appreciation to Christiane Baltaxe whose interest and efforts made my work with MB possible. I also want to thank Breyne Moskowitz and Susan Curtiss for their helpful comments, and Harold Gordon for the use of his bilingualism questionnaire. Finally a heartfelt thanks to my language consultants, Amnon Gordon, Janet Hada, Norman Shulman, Luminza Nicolascu, Klara Gal and Edward Bat.

References

- Critchley, M. 1974. Aphasia in polyglots and bilinguals. Brain and Language, 1, 15-28.
- Galloway, L. 1976. Bilingual Aphasia: Issues in Bilingual Aphasia, A Case Study, and Experimental Evidence for a Theory of the Lexicon. M.A. Thesis. Department of Linguistics, The University of California at Los Angeles.
- Hinschelwood, J. 1902. Four cases of word blindness. The Lancet, 1, 358-363.
- Paradis, M. (in press). Bilingualism and aphasia. In H. Whitaker & H. Whitaker (eds.) Studies in Neurolinguistics 3. New York: Academic Press.
- Pitres, A. 1895. Etude sur l'aphasie chez les polyglottes. Revue de Médecine, 15, 873-899.
- Ribot, T. 1906. Diseases of Memory. London: Paul, Tranch, Trubner.
- Wechsler, A. 1977. Dissociative Alexia. Archives of Neurology, 34, 257.

III

LINGUISTICS APPLIED TO LANGUAGE TEACHING AND PLANNING

BILINGUAL EDUCATION: A NEW CHALLENGE TO OLD PRIORITIES

Robert J. Di Pietro
Georgetown University

For most of its history, American public education has been impervious to substantial changes or adjustments in its root beliefs and life values. Despite considerable differences in the ethnic composition of local populations across the country, individual state systems have promulgated similar charter myths deriving from the dominant white Anglo-Saxon Protestant culture of the northeastern part of the country. Even the native of New Mexico attending a public school in that state of the distant southwest is expected to believe that the Massachusetts Pilgrims were the fathers of us all, that American history is nothing more than an extension of English history, and that there was never any question of what language to speak throughout the colonial period of our country. As a native of New York State and a product of the same educational syllabus, I never learned that the first public announcement of the signing of the Declaration of Independence was in the Staatsbote, a German-language newspaper of Pennsylvania or that there were at least 16 different ethnic groups in the Dutch colony of New Amsterdam at the time that it fell to the English. No one ever taught me that Thomas Jefferson, the most talented of all American presidents, was inspired by the political writings of Philip Mazzei which he translated from Italian into English. One of Mazzei's papers includes the Italian original of the phrase, 'all men are created equal' (see Marchione 1975). Practically nothing was ever reported in my history books about the literary achievements of the Cherokees. Only by going through the printed testimony given in Washington, D. C. at the hearings for the Bilingual Education Act some ten years ago did I learn that the Indians of the Oklahoma Territory had obtained a higher rate of literacy in both English and Cherokee than the local white population during the early years of this century. I wonder how many of this audience know that English was the minority language in several parts of the American southwest as late as 1948. Such facts are rarely, if ever, discussed in our textbooks.

The Bilingual Act of 1968 and the Lau vs. Nichols decision of 1974 stand out as landmark events. These recent actions represent the first serious challenges to the exclusive use of English in our schools and the traditional orientation to mainstream charter myths. Not even the onslaught of millions of immigrant children from Italian and east-European stock around the turn of the century was able to make a noticeable impact on the existing educational establishment. In those days, newcomers were simply expected to acculturate--not in the direction of the majority of Americans who have always been ethnically diversified, but in accordance with the image of what Americans should be, culturally and linguistically. This image was projected by a small, yet influential, elite of textbook writers and teacher-trainers. While each new wave of European newcomers did acculturate, they did not do so without developing their own mindsets about American institutions. Among the Italians, American public schools came to be seen as instruments of alienation from family values. The founding of the Parent-Teachers' Association by Angelo Patri, himself an Italian American, was one effort to counter this growing sense of alienation. Unfortunately, P-TAs have had little to say over the years about important matters such as textbook selection or choice of subject matter.

Another characteristic of public education in the United States is its remarkable ability to perpetuate itself. Let me give an illustration by drawing your attention to an incident that happened in a section of Chicago a few years ago. The student body of a local school rebelled against their principal. These students were predominantly Spanish-speaking and they accused the principal of being insensitive to their culture and language. The principal, who was of Polish extraction, fought back to save his job. The newspaper reporters pointed out that the neighborhood had been a Polish one before it became a barrio latino. Apparently, the principal had grown up in the same neighborhood when the schools were being run by persons of yet an earlier ethnic group. By the time he had reached his present position of administrator, his own people had all moved away. Now he found himself the defender of the same establishment which had been oblivious to his own heritage. The plot of this drama continues unchanged throughout the country. Only the actors are different. I am tempted to agree with Giuseppe Fiori (1971) who sees the victory of the ruling classes over the subjected ones as being complete when those in command finally get the masses to believe in the

upper-class value system so strongly that they label it 'common sense' and are willing to defend it at all costs to their own welfare.

Now there is a crack in the educational superstructure. It was put there, however, not by any direct action of the P-TA or the National Educational Association or any band of enlightened university professors. It was put there inadvertently by several politicians who saw the vote-getting potential of associating the inability to speak English with the poverty-level incomes of large segments of our population. Whatever its effects will ultimately be on American public education, the Bilingual Education Act was at its initiation an anti-poverty measure. In the simplest of terms, its supporters argued that no American should be denied access to earning a living because of language difficulty. Since schools are supposed to prepare children for productive lives in society, it logically follows that their needs to communicate should be met.

Of course, on such terms, bilingual education need not be any more than a program to teach English as a second language. Educators began to talk about 'transitional bilingualism', whereby the actual use of two or more languages in the schools becomes a phase necessarily gone through in taking children from their home languages to fluency in English. It was the Lau vs. Nichols decision of the Supreme Court which gave the legal assurance that the home language (and culture) would be fully represented throughout the child's education. While even ESL programs provide a challenge to the American public school system, their presence in it is nowhere near as upsetting as that of a full bilingual program. ESL teachers can be placed in the system as additional faculty. As is the case in many American schools, a special room can be set aside for ESL to receive students who have been having difficulties understanding their 'regular' classroom teachers. In a bilingual program, even a transitional one, course content must be presented in one or more languages in addition to English. The other-language instructor cannot be placed off in a corner somewhere. He or she must be invited to share in decisions regarding subject matter, textbooks and supporting materials. In the utopian bilingual program, all subjects are teachable in more than one language. While I know of no school anywhere that has achieved this goal, I can talk about what is now happening in some schools which have started along this path in the U. S.

The Oyster Elementary School of Washington, D.C. is as near to a balanced bilingual school as any I have seen. Each classroom has two teachers--one English-speaking and one Spanish-speaking. The teachers station themselves at opposite ends of the room and instruction goes on in both languages simultaneously. The children may sit facing either of the two teachers and are free to change sides at any time, going from English to Spanish or vice versa. The D. C. Department of Education is rather proud of this school. There is a good deal of esprit among the children, the teachers and the parents. In comparison with the generally poor record of instruction in D. C. schools, the Oyster School stands out as a kind of model program.

Other schools in the greater Washington area are now beginning to experiment with forms of bilingual education. At the Key Elementary School in Arlington, Virginia, students are permitted to enroll in one of six possible courses of language instruction: Spanish, Korean, or Vietnamese for native-speaking children, or one of the same three languages for children who do not speak them. The program is supported by the community and is blessed with a dedicated corps of teachers and administrators. The Key Program is, however, not yet fully multilingual or multicultural. Only basic mathematical functions are presented in the three languages and the cultural/historical coverage tends to be limited to areas of the world where these languages are spoken by the majority. The next step would appear to require a rendering of the basic curriculum multilingually as well as multiculturally.

In my visits to the Oyster and the Key Schools, I found no indication of the divisiveness that bilingual education is sometimes accused of fostering (see, for example, the New York Times editorial of Nov. 22, 1976 entitled 'Bilingual Danger'). The children in both schools appeared to be adjusted not only in the classroom but on the playground as well. The diversity of cultures and languages does not seem to provide any unsurmountable barrier to cooperation. Of course, it is too early yet to measure the impact on the children's long-term academic performance. At present, we have little more to go on than the results from the verbal and mathematics tests customarily given to all American school children. Some non-English-speaking children score low on such tests, but that should not be surprising. The teacher-corps at Key Elementary School plans to develop achievement tests which reflect more

equitably the actual language usage expected of bilingual children. The Oyster School has attempted to keep track of how well the children do when they go on to higher levels in the same school system.

In my opinion, the most imposing argument against bilingual education centers on the claim that extensive exposure to more than one language in a school program may retard language development in the child. Will children educated bilingually always have vocabularies which are more restricted in both languages than monolingual speakers of either language? Does bilingual education contribute to interlingual interference and retardation in concept-formation? Peal and Lambert (1962) have compiled a report on tests dealing with bilingualism and intelligence, but we cannot yet give any answers to these questions which would hold for all circumstances and variables. One of the major factors influencing the educational achievement of children in bilingual programs--at least in the U. S.--seems to be the attitudes of the parents and the community. We should keep in mind that American public education is in trouble on its own terms. The general levels of achievement in English and math are so low that some school systems are considering the requirement of a literacy test for graduating seniors. Failures have not been given willingly because holding back a child at any grade level is costly to the taxpayers. In most American schools, teachers are strapped with paper work which robs them of the time they can devote to their students' needs. Against such a background, the school with a bilingual program stands out as unique in the concern it has for the children's general academic performance.

The cultural component of bilingual and multilingual programs has only recently been taken into consideration. Educators are beginning to appreciate the extent to which instruction in a language entails the diffusion of the life values of the speakers of that language. The result of a study done by Edgerton and Karno (1971), regarding community preferences in health care, strongly suggests that speaking a language is a more reliable indicator of the cultural values of a people than their professed membership in an ethnic group. Among persons who identified themselves as Mexican-Americans, those who spoke English tended to share opinions about medical care with the Anglos of the area while Spanish-speakers of the same ethnic group were inclined to the Latin viewpoint on the subject.

Awareness of the many interconnections between language and cultural commitment inspires some educators to revise their school curricula, while for others, the threat to their own life values is too great to lead to action. I know of two contiguous counties in the state of Virginia which have taken diametrically opposed positions when faced with the possibility of introducing a bilingual curriculum. Where it is being openly experimented with in one county, the other has mounted a campaign to keep it out. The unfortunate fate of FLES programs in our schools is a vivid example of how a school system reacts to a language and culture which it interprets as 'foreign'. When the time came to cut school budgets, foreign language instruction was one of the first items to go. In fact, foreign language enrollments have been going down across the country at the same time that travel by Americans to other countries has actually increased.

Several things must happen if bilingual education is to succeed in the United States. First of all, multilingualism must be freed of its association with poverty and ignorance. Ironically, bilingual education, as it has been legislated, has contributed to the stigma of being linguistically and culturally diverse from elitist-generated notions about the mainstream. With the requirement that a community show itself to be at the poverty level in order to qualify for funds from the bilingual act, the resulting educational programs are labeled as 'compensatory education'. On a social-economic continuum from rich to poor, monolingual English speakers stand at the rich end, monolingual speakers of other languages are grouped at the poor end and bilinguals are somewhere in-between, on their way to monolingualism in English.

One strategy to break this lock-step would be for the community of foreign language teachers to join forces with bilingual educators to convince the public of the value of knowing more than one language, whatever one's culture might be. From a social disgrace, multilingualism must move to identification as a national asset. Business leaders, for example, should be assured that multilingualism is no threat to their industries. Looking back in time, we find that Henry Ford was very much in favor of the melting-pot theory because he feared that fostering linguistic and cultural diversity would be harmful to the success of his mass-assembly plants. There are already signs that the industrialists of today are beginning to recognize some benefits of diversity among workers. At higher

levels of industry, the value of speaking more than one language has already been appreciated. Some American firms have begun to train their executives in other languages.

Another event that must take place for bilingual education to become a respected alternative to mainstream education in the U. S. is the recognition of its benefits to the local community. We can no longer afford the luxury of letting our inner-cities decay. Already the flight to the suburbs has stopped and many young mainstream-oriented adults are moving back into the cities. Once back in the cities, they encounter the ethnic neighborhoods and find themselves interacting with people who are linguistically distinct from them. The educators in inner-city schools have the opportunity to develop truly innovative programs for their changing student populations and correct some of the abuses of the earlier prescribed curricula.

Yet a third happening must occur to make bilingual education a success in the U. S. and that is to assure its availability to all Americans, regardless of their income levels. As it now stands, middle-class, affluent families are not entitled to bilingual programs. Why should my own children be deprived of the opportunity to learn another of the languages spoken by their fellow Americans? If I had a choice, I would like them to study Italian because of my own heritage. However, I would be happy if they had the opportunity to learn some Chinese or Korean. From what I have seen of the instruction they now get in the so-called 'language arts' at the elementary level, exposure to Chinese or some other language would have no detrimental effect whatsoever on the work they are expected to do in English. My dream is of a time when the elitist traditions in American education will be broken and new materials will be developed which will reflect the rich ethnic heritage of our country.

Theoretically speaking, bilingual education has a dual purpose: it answers the recognized need of education to be sensitive to the life experiences of the community and it also provides children with skills they can profitably use to live in a shrinking world. However, some of the problems facing bilingual/bicultural education are enormous. Bilingual education threatens the monolingual establishment with its sanctioning of alternative languages and diverse life styles. It has not found a solution to how to structure its own curriculum and how to prepare its teaching personnel.

It even lacks a definition which can be understood by all people. A survey of congressmen in Washington taken by several of my graduate students a few years back (see Pratt, et al., 1976) revealed that few legislators could provide definitions either for bilingualism or for bilingual education despite the fact they had voted for or against an act allocating millions of dollars to bilingual education.

We must not, however, become discouraged. The alternative to bilingual/bicultural education is to continue on the path to mediocre school programs for all American children. That is an alternative which we must never accept if we dare to call ourselves responsible educators.

REFERENCES

- Edgerton, R. B. and M. Karno, 1971. "Mexican-American bilingualism and the perception of mental illness", Archives of General Psychiatry, vol. 24, no. 3 (March).
- Fiori, Giuseppe, 1971. Antonio Gramsci: Life of a revolutionary. New York: E. P. Dutton.
- Marchione, Margherita, 1975. Philip Mazzei: Jefferson's "zealous whig". New York: American Institute of Italian Studies.
- Peal, Elisabeth and W. Lambert, 1962. The relation of bilingualism to intelligence. Psychological Monographs: General and Applied, vol. 76, no. 27.
- Pratt, Yvonne K., Randolph M. Scovil, N. Caroline Tryon and Kristie Twaddell, 1976ms. "Bilingualism in congress". Georgetown University, Wash., D. C.

PROCEDURES INVOLVED IN DEVELOPING LITERACY MATERIALS FOR A PRELITERATE SOCIETY

Ronald C. Morren
Summer Institute of Linguistics

Leon E. Williamson
College of Education, New Mexico State University

INTRODUCTION

There are millions of people in the world who have little chance to develop their abilities to full potential because they lack the basic educational tools of reading and writing. Many of these "millions" are speakers of languages which have not yet been reduced to writing. As mass communications decrease boundaries among men, opportunities increase to be scientific in our efforts to write the languages of preliterate societies. The following describes the research done in one such society--the Sama Bangingi of the Republic of the Philippines.

Writing the language of a preliterate society is more meaningful for the target group if the linguist avoids his own ethnocentrism and focuses on their's. Gray (1956:12) wrote, "research shows that initial progress in learning to read and write is quickest in the mother tongue." This conclusion is based upon research done in Ghana, Republic of the Philippines, and New Caledonia. Neijs (1961:47) supports Gray's conclusion when he writes:

In teaching adults to read it is always best for them to begin in the mother tongue. It has also been demonstrated that an individual who is literate in his own language can learn to read and to understand a second language more easily than an illiterate can.

Modiano's research corroborates Gray's and Neijs' findings. Modiano (1968:42) concludes:

...youngsters of linguistic minorities learn to read with greater comprehension in the national language when they first learn to read in their mother tongue than when they receive all reading instruction in the national language.

CONTRIBUTIONS FROM OTHER DISCIPLINES

Anthropological and Sociological Considerations

Developing meaningful literacy materials requires more than a knowledge of linguistics. A knowledge of anthropological and sociological techniques can help a linguist reduce his ethnocentrism and to be more sensitive to what members of a preliterate society value. It will help him in relating the target group's social and cultural dynamics to linguistics. This sensitivity

will aid the linguist in choosing meaningful topics for literacy materials which will be more likely to motivate members of a preliterate society toward literacy.

Anthropological theories are important in selecting and developing literacy materials if the linguist wants the materials to take into account the features of the society which may be universal, which may be present in that certain type of society, or which may be specific only to that society.

Appropriate literacy materials will reflect the composition, forms, structure, and functions of human groups. These depict human togetherness or human grouping; they show how patterns of association develop and change over a period of time. Carr (1948:3-4) distinguished two main types of such patterns. (1) background patterns, and (2) foreground, or focalized patterns.

Background patterns reveal relationships or groupings regarded as the settings for events. They include: (1) Primary groups which are small, intimate, face-to-face, unorganized and spontaneous. They may be pair groups, families, play groups or rural neighborhoods. (2) Casual groups such as crowds, audiences, etc. (3) Purpose organizations which include families, business concerns, athletic teams, church congregations, governmental bodies, armies, etc. (4) Status groups or social classes. (5) Residence-subsistence groups which are communities varying in sizes. (6) Total societies such as tribes and nations. (7) Culture groups which are separate tribes or nations similar in culture. (8) Races which are people grouped together on the basis of cultural definitions or biological characteristics.

Focalized patterns are part of patterns rather than the whole. We think of farmers not only as an occupational group, but in relation to the nation's food supply. We think of drought as a threat, of its effects on our food supply, on our personal security, on inflation, etc. It is a situation. A situation is a focalized pattern of social relationships regarded as a source of actual or potential experience.

Sociolinguistic and Psycholinguistic Considerations

Sociolinguistic considerations involve an awareness of a society's behavior in relation to its language. The written material of more prestigious or dominant languages (i.e. national language or regional trade language) frequently exerts pressure on minority groups to conform. A minority group is usually more likely to accept the linguist's decisions concerning its language's written form if he utilizes an alphabet which parallels the prestigious language where possible, rather than an alphabet which diverges from it. Therefore, the influence of neighboring societies concerning type of script, choice of graphemes, and spelling patterns and conventions must be considered by the linguist.

In addition to the linguistic analysis and the sociolinguistic pressures, is the consideration of psycholinguistics, or the way an individual native speaker feels about how his language is

put together. To discover the native speaker's reaction to the phonological elements of his language, the analyst must be alert to the native speaker's intuitive reactions. Of course, the trained linguist cannot avoid having personal convictions about how the language under study should be written but he must not let these convictions dictate by imposition aspects of the orthography which are diametrically opposed to native reaction. To do so may jeopardize the future use of the orthography or written materials which utilize that orthography and negate the investigator's work.

LINGUISTIC ANALYSIS

After anthropological, sociological, and psychological perspectives have guided the selection of materials, a corpus should be chosen which has the phonological, morphological, and syntactic frequencies to justify inclusion in the literacy program. For Sama Bangingi, only materials essential for developing a defensible scope and sequence of skills for teaching reading with culturally relevant and meaningful literacy features were included.

Since phonology is the basis for orthography, it will be discussed first.

Phonemes

Vowel phonemes have allophonic pairs which appear in stressed and unstressed syllables. They are described as to position and rounding in the following chart.

	Front	Central	Back
High	/i/ [i] [ɨ]		/u/ [u] [ʊ]
Mid	/e/ [e] [ɛ]		/o/ [o] [ɔ]
Low		/a/ [ʌ] [a]	

The stressed allophonic vowels with examples from the language are: [i] as in [bʌ.'li.ʊ] 'wind'; [e] as in ['pe.kɔ^k] 'crooked arm'; [a] as in ['la.nu^t] 'abaca'; [u] as in ['bu.ʌs] 'rice'; [ɔ] as in ['so.ʌ] 'snake'. The unstressed allophonic vowels are: [ɨ] as in ['la.ɨ] 'plate'; [ɛ] as in ['po.lɛʔ] 'return home'; [ʌ] as in ['pe.ʌ^t] 'red'; [ʊ] as in ['ka.yʊ] 'firewood'; [ɔ] as in ['ben.kɔɪ] 'hem'.

In addition to the single vowels, geminate clusters occur. These identical vowels occur contiguous to each other and each is the peak of a separate syllable. The following examples show minimal pairs of words contrasting the single vowel and the geminate vowels. /a.ta.a/ 'distant'; /a.ta/ 'slave'. /pa.gu.un/ 'is floating'; /pa.gun/ 'an island made of floating vegetation'.

There are seventeen consonant phonemes. They are described by the following chart:

	Bilabial	Alveolar	Alveo Palatal	Velar	Glottal
Voiceless	p	t		k	ʔ
Stops					
Voiced	b	d		g	
Fricative					h
Nasals	m	n	ɲ	ŋ	
Affricative					
Voiced			ɟʝ		
Sibilant		s			
Lateral		l			
Semi Vowels	w		y		

All seventeen consonants occur freely in syllable onset. Only /b/, /d/, /g/, /p/, /l/, /s/, /m/, /n/, and /j/ occur freely in syllable coda. The phonemes /p/, /t/, and /k/ each have allophonic variations: [p], [t], and [k] occur freely as syllable onset, but occur syllable coda only before /s/ or before a vowel in the following word of the same pause group. [p], [t], and [k] occur in other positions, that is, pause-group-finally and before consonants other than /s/ in the same word or as the first segment of the following word.

/m/ and /n/ may occur as syllabic consonants in a stem initial homorganic cluster. /ɲ/ occurs only as syllable onset and does not occur as part of a consonant cluster.

/y/ and /w/, as syllable onset, do not occur as the second member of a consonant cluster. As syllable coda they occur word medially in a word composed of a reduplicated stem, or word finally.

/h/ as syllable coda does not contrast phonemically with its absence, i.e. it contrasts with other consonants in this position in the same way as silence. Word final [h] is interpreted as a transition phenomenon between a vowel and silence. /ʔ/, which occurs as syllable onset and coda, does not occur syllable coda word medially in a word stem but does occur syllable coda word medially when the stem is affixed. /ɟʝ/ occurs only in syllable onset position. In a consonant cluster it occurs only after /n/ or /d/.

Sequences of two identical consonants, or geminate clusters occur with all the consonants except ɲ, ɟʝ, ʔ, and h. Phonetically, geminate consonants are lengthened consonants. Each belongs to a distinct syllable and must be given its full value. However, no break in speech occurs interjacent to the geminate consonants.

Syllabic Patterns

The syllable is the minimum phonological unit which occurs in isolation. There are five syllabic patterns in Sama Bangingi: V, C, VC, CV, and CVC. Of the five, CV and CVC are numerically predominant.

Stress has a high degree of predictability. Primary stress occurs on the penultimate syllable of most words. When a one

syllable suffix is attached to the stem, the stress is moved one syllable to the right, remaining penultimate.

A phonological word in Sama Bangingi is made up of combinations of the five syllabic patterns united by a single primary stress. There are restrictions regarding the way syllables can combine to form phonological words. They are: (1) V only occurs following a CV syllable. (2) C: (a) stem initial. (b) preceded only by CVC prefixes on the stem. (c) cannot be followed by VC, V, C, or CV syllables. (3) VC: (a) can only be preceded by CV syllable. (b) cannot occur word initial. (c) cannot be preceded by a C, VC, or CVC syllable. (d) cannot be followed by a V or VC syllable. (4) CV cannot be followed by a C syllable. (5) CVC: (a) can be followed by C syllable only when CVC is a prefix. (b) cannot be followed by a V or VC syllable.

Morphology

A word spoken by the Sama Bangingi is a segment of speech which begins and ends with a point of potential pause and consists of one or more morphemes one of which is a stem. There are bound and free morphemes. The major stem classes are: verbs, nouns, and adjectives. Stems can be affixed in various ways resulting in a slight change or different shade of meaning. All contentive simple stems which are not particles or pronouns can be inflected.

Verbal Focus System

There is a phenomenon in the languages of the Philippines which shows the relationship between the verbal comment and the topic phrase called focus. The verbal focus system is basic to grammatical structure and is found in three verbal categories: (1) voluntary and involuntary action. (2) simple and distributional mode. (3) imperative mode. The term focus is used as a label for the topic-indicating affix on the verb. The focus inventory is the range of specific focus affixes and their resultant permutations which are possible for a particular class of verbs. Within the range of focus possibilities there are six classes: (1) object (2) actor (3) instrument (4) time (5) location (6) destination. All verbal categories do not take all foci. The predominant focus in verbal sentences is object focus. The next most commonly used focus is actor. All other foci account for less than 10% of the verbal sentences.

Thus, most verbal sentences used in the Sama Bangingi primer will be of these two types. Examples:

Bono?	/	-ku	/	edo?	/	maka ladiɔ.
obj focus vb	/	nonfocus actor	/	obj focus	/	nonfocus Inst
fight	/	I	/	dog	/	with knife

'It is the dog I fight with a knife.'

Amono?	/	aku	/	edo?	/	maka ladiɔ.
actor focus vb	/	focus actor	/	nonfocus obj	/	nonfocus Inst
fight	/	I	/	dog	/	with knife

'I fight the dog with a knife.'

Verbs are constructed by inflection and by derivation. Verb stems are never spoken in a stream of speech without some form of affixation. Verbs can be derived from adjective stems by adding the prefix pa- to the adjective stem. In actual usage, these derived verbs are further affixed according to the rules for focus.

/liŋkat/ → /paligkat/ /ettom/ → /pa'ettom/
'beautiful' 'to beautify' 'black' 'to blacken'

Verbs can be derived from noun stems by adding the prefix ag- to consonant initial stems and by adding the prefix ay- to vowel initial stems.

/luma?luma?/ → /agluma?luma/ /ulan/ → /agulan/
'playhouse' 'to play house' 'rain' 'to rain'

Adjectives

Most adjective stems are usually not uttered without one of the a-, -in-, (sometimes ni-), -an, or ma- affixes. There is a very small number of adjective stems which are not inflected. The most common adjectival affix is the prefix a-. /haap/ → /ahaap/ 'good'. Another basic affix is the -in- infix or the ni- prefix. /karu?/ → /kinaru?/ 'sleepy'; /?uban/ → /ni?uban/ 'having grey hair'.

Derived adjectives are derived from noun stems by the addition of -an to the noun stem. These words are descriptive or possessive. /lisu?/ → /lisu?an/ 'lazy'; /daya/ → /dayahan/ 'rich'. Also, the ma- prefix is derivational for adjectives. /?asin/ → /ma?asin/ 'salty'.

Equalitive relationships are indicated by the affixes da-, saŋka-, or sa-, with da- being most common. da- can occur with adjective stems or may be derived from noun stems; the equalitative topic must be plural.

/bissara/ → /dabissara/ /to?a/ → /dato?a/
'language' 'same language' 'old' 'equally old'

The affix saŋka- is used with verb stems to indicate pairs of activities which keep pace with each other. sa- is used to indicate relationship. Resultatives are: maka-, aN-, mag-, syllable reduplication, and ka-...-an. maka- makes a comment about the causer of the state described. aN- indicates transition to the state or stage indicated. It occurs with adjective stems and with noun stems which refer to contrastive stages, such as stages of growth. The prefix maga- with reduplication of the first consonant-vowel sequence of the stem indicates an increasing degree of the condition or state described; /mehe/ 'big' /magmemehe/ 'getting larger'. The adjectival affixation ka-...-an indicates the topic is affixed by the state of the thing to which the description applies.

Nouns

Nouns as a stem class consist of simple noun stems which can be inflected. Nouns may also be derived from verbs and adjectives by affixation on the stem. Reduplication of the noun stem acts as a diminutive; /luma?/ → /luma?luma?/; 'house' → 'playhouse'.

Noun stems may be inflected by affixation which results in a slightly different meaning for the stem; /luma?/ → /kaluma?an/; 'house' → 'village'.

State of being nouns are derived by adding pag- or paN- prefix to the verb stem. Nouns as agents are derived from verb stems by adding paN- to the verb stem. Reduplication of a syllable takes place in most of these words with vowel harmony occurring in vowel initial stems; /bono?/ → /pamomono?/; 'to fight' → 'a person who is always fighting'. All nouns of this category do not undergo a morphophonemic change but are prefixed with pa- plus reduplication; /tuli/ → /patutuli/; 'to sleep' → 'a person who is always sleeping'.

Nouns as instruments are derived from verb stems by reduplicating the first syllable of the verb stem; /sapu/ → /sasapu/; 'to sweep' → 'broom'. Nouns may be derived from adjectival stems by adding the prefix ka- to the stem.

Pronouns

Pronoun stems consist of one morpheme and are not inflected. or derived. There are two definite sets of pronouns. Set I includes the pronoun as topic and the pronoun as object. Pronouns of Set I are free morphemes. Set II pronouns include those where the actor is not in focus and possessive forms. These are affixed to other stems.

	SET I	SET II
SING.	aku	-ku
	ka?a	-nu
	ia	-na
PLURAL	kami	-kami
	kita	-ta
	kitabı	-tabı
	ka'am	-bi
	sigam	-sigam

Demonstratives constitute a word class. There are two sets. The first set expresses: (1) /itu/, /itia?/ 'this' by the speaker. (2) /ilu/ 'that' by the hearer. (3) /ina?an/ 'there' far away from speaker and hearer but in sight. Set two: (1) /mintu/, /mintia?/ 'here' by the speaker. (2) /mau/ 'there' a short distance away from the speaker. (3) /me?/ 'there' farther from the speaker but usually in sight. (4) /?ele?me?/ 'there' very far out of sight. Another word /?ania?/ 'existing; there is; there are' is used as a demonstrative.

Interrogatives substitutes for words or phrases and may be inflected. Sama Bangingi interrogatives are: /?aiyan/ 'what'; /saiyan/ 'who' both of which substitute for a noun or noun phrase. /agayan/ 'why' substitutes for a verb or verb phrase. The word /sumilan/ 'when', may replace a time word or phrase. The concepts of 'how many, how much' are expressed by /pila/ which substitutes for a numeral or quantifier phrase. The words /saiga/, /saigahan/, /siniiga/, /siniigahan/ express 'how to do; how to make' and may substitute for a demonstrative word or phrase. The pairs /maiga/, /maigahan/ 'where; which' and

/miniŋga/, /miniŋgahan/ 'from where' substitute for a demonstrative phrase or an identifier phrase. The pair: /paiŋga/, /paiŋgahan/ 'where going' substitute for a locative or directional phrase or word.

The Sama Bangingi particles are divided into a number of different classes. They are characterized morphologically by their function. The following are some adverbs as particles: /lagi?/ 'yet', /to?od/ 'very, intensifier', /sadza/, /sidza/ 'only', /kono?/ 'reportedly'. Some prepositions as particles: /ni, i/ 'to--indicates destination', /ma/ 'at; to--indicates an object or station', /min/ 'from--indicates sources', /tuđu/ 'toward', /maka/ 'with'. A few conjunctions as particles: /sogo?/ 'but', /atawa/ 'or', /mandzari/ 'and then', /baŋ/ 'if; when'. Some negatives as particles: /da?a/ 'prohibitive', /?insa?/ 'none', /duma?in/ 'it is not the case that; contradiction', /?iŋga?i/ 'negates state of things; no; not so'. The most common affirmative is /?aho?/ 'yes'.

The following are some specialized words or particles: /si/ 'person marker', /na/ 'current marker; now', /bai/ 'past tense marker', /maŋa/ 'plural marker; all'.

Basic Sentence Structure

A complete grammar for Sama Bangingi is not in the scope of this paper. The concern of this paper is with the structure of utterances for a beginning reading program. Sentence structure is important for naturalness and readability. In Sama Bangingi there is a basic construction which consists of two components: a topic and a comment. These may be verbal or non-verbal.

The primary comment of a verbal sentence is the verb. The total comment consists of the verb plus the non-topic components of the sentence. The verb is always affixed to indicate which component is in focus. The topic of a verbal sentence is filled by a Set I pronoun, a noun, or a noun phrase unmarked by one of the prepositions ma, min, ni, or maka. If a noun phrase is introduced by one of these four preposition-like particles, it is a non-topic sentence-level phrase.

In non-verbal topic-comment sentences, the comment may be filled by a classifier (number, identifier, measurement, etc.) attributive adjective, noun, noun substitute, or locative phrase. The topic may consist of a noun, a noun substitute, or a noun phrase. Example:

<u>Comment</u>	<u>Topic</u>	<u>English Gloss</u>
aliŋkat	anak-nu	'Your child is
'beautiful'	'child-your'	beautiful.'

There may be more than one comment phrase in the topic-comment type sentence. This occurs with locative and superlative comment in non-verbal sentences. Example:

<u>Comment₁</u>	<u>Topic</u>	<u>Comment₂</u>
Atuas	kayu itu	min kayu ina?an.
'hard'	'wood this'	'from that wood'

'This wood is harder than that.'

An example of a verbal sentence:

<u>Comment₁</u>	<u>Topic</u>	<u>Comment₂</u>
Ajalubak	aku	edo?
'beat'	'I'	'dog'

'I beat the dog.'

Verbal sentences can be greatly expanded but will basically consist of a topic and one or more comments. These sentences can be described further according to their syntactic sentence types. The following are examples:

Interrogatives

Mainga luma? siga?am?

Where house-their

'Where is their house?'

Pila siin saingilu?

how much money bananas those?

'How much are those bananas?'

Imperatives

Too?in ma?aku.

show it to me

'Show it to me.'

Exclamations

Aliqkat to?od.

'A very beautiful (person, place or thing)

Statements

Atuli anak-ku.

sleep child-my

'My child is asleep.'

Iyga?i aku abaya amajan.

neg. I want to eat

'I do not want to eat.'

For additional discussion of the basic linguistic analysis of Sama Bangingi see Morren (1977).

ORTHOGRAPHY AND READING

The simplicity of Sama Bangingi phonology makes a one-to-one relationship between phonemes and graphemes a simple matter. Ashley (1973:88) wrote: "The basic principle which guides the orthographer in his work is. ... that each sound (phoneme) must have one and only one distinctive symbolization." Ashley does recognize other considerations which may dictate that this principle be relaxed. Johnston (1967:3) identifies three factors which may relax this principle. They are: (1) native speaker's attitude toward phonemics and phonetics of his language. (3) The analyst's psycholinguistic perceptions concerning orthographical symbols and conventions.

The choice of the orthography for Sama Bangingi was influenced by Bangingis literate in another language, especially Pilipino. Since Pilipino is the national language of the Philippines, its orthography affected the orthographic choices for Sama Bangingi. The orthography of Pilipino is phonemically based. The educated Bangingis are the leaders and hold persuasive influence over their illiterate neighbors. These leaders insisted upon transitional consonants for some vowel combinations. Even though it means extra spelling rules, this compromise will further facilitate the transfer or reverse transfer whether one first learned to read in Pilipino or his own vernacular. This compromise is illustrated in the vowel sequence ai and au. These are infrequent in Pilipino but very common in Sama Bangingi.

They will be written with no transitional consonant to separate them. However, the following vowel sequences will utilize the transitional consonant even though phonemically it is absent.

ia→iya; ua→uwa; iu→iyu; ea→eya; oa→owa; eo→eyo.

The alphabet-phoneme correspondence for Sama Bangingi is
 /a/: a. /b/: b. /d/: d word initial, r intervocalic. /e/: e.
 /g/: g. /h/: h. /i/: i. /dz/: j. /k/: k. /l/: l. /m/: m.
 /n/: n. /ŋ/: ng. /ñ/: ny. /o/: o. /p/: p. /s/: s. /t/: t.
 /u/: u. /w/: w. /y/: y. /ʔ/: ʔ. The phonemes /ñ/, /ŋ/, and /dz/ are represented by their respective graphemes (ny, ng, j) as that is the way those phonemes are represented in the national language.

THE READING PROGRAM

The language analysis and the orthography provide the basis for the reading program. Besides having culturally and linguistically relevant materials, each lesson is planned so new material is introduced systematically and previously taught skills are sufficiently reviewed. Prior to selecting materials for each lesson, all skills which are to be included in the scope of the instructional program are reviewed. This includes a survey of the orthography and morphology so that all symbols and words relevant to teaching will be included. Frequently-used sentence patterns are also listed to be included in the material. These lists are then programmed for maximum use and productivity through the use of letter and function word frequency counts, contrastive visual shapes, syllabic patterns and sentence types.

This controlled sequence becomes the basis for all the lessons in the literacy program. The new symbol is taught by means of key words plus various exercises. It is reinforced in meaningful connected material and handwriting practice.

Scope and Sequence

A total inventory of all elements that must be taught is necessary for a scope to be developed. All graphic symbols must be accounted for with those of the highest frequency being taught first to enable the use of meaningful story material to be read by the students in the very first lesson of the primer. A frequency count of graphemes as used in various texts warranted teaching the vowels a, i, u in the preprimer with a review of them in lesson one of the primer, whereas vowel e is taught in lesson 18 and o in lesson 22. Initial consonant t is introduced in lesson 2; final t in lesson 5. Initial k is taught in lesson 3; final k in 10. Frequency of final ng warranted introducing it in lesson 15, while initial ng is taught in lesson 23. The glottal stop is taught in lesson 14, and so on. After every six lessons there is a review.

Structure word frequency indicated that the structure word itu should be presented in lesson 1; iya in 8; pag- in 12; yuk in 15; etc. In all, 6 preprimer lessons and 45 primer lessons were developed for the Sama Bangingi. In addition to the drill on the new symbol being taught and writing practice, each primer

lesson contained one or more native-authored story. For example, for lesson forty, the new letter to be taught was word medial, intervocalic *-r-*. The native authors were asked to choose a "keyword" which used the new symbol in the position in which it was being taught and then to write a story about that keyword. The keyword was to be easily picturable so that a picture (or drawing) could accompany the story. The keyword chosen for lesson forty was *baras-baras* which means 'canebrake'. The following short story about 'canebrake' was written and included in the Sama Bangingi primer.

BARAS-BARAS SI KELLOTO'

Si Kelloto' atebot, usahana magkillo' baras-baras ma deyom bangkaw. Aheka' baras-baras aniya' alangkaw maka abontol.

Kami agbilli baras-baras pagsasak deyo' luma' maka pagsoksok luma'-luma' etek. Aheka' etek ma lahat kami angkan pinag-apas baras-baras si Kelloto'. Kasehean amilli baras-baras pagsasak sinayul sali' na manga sayul lara, labu, patula', talum maka bawang.

Bang manga sinayul taga sasak ingga'i tasod e' manga sattuwa. Maka bang deyo' luma' taga sasak ingga'i isab alimmi'. Damikkiyan isab etek bang taga sasak ingga'i akanat entollona. Ampa entollo ma buwatitu ahunit. Minsan da manga aa min kalahat-lahatan magbilli da baras-baras si Kelloto' sabab heka' kapaiddahanna.

KELLOTO'S CANEBRAKE

Ambitious Kelloto's work is gathering canebrake from inside the swamps. There is a lot of canebrake, tall and straight.

We buy canebrake for fencing under the house and for patching the duck house. There are a lot of ducks at our place so we are getting a lot of canebrake from Kelloto'. Some buy canebrake for fencing vegetable gardens for vegetables like pepper, white squash, green squash, egg plant and onions. And if there is a fence under the house, it is not so dirty. Also, if the ducks have a fence, they do not scatter the eggs. Eggs are expensive now. Even a lot of people from a lot of places buy Kelloto's canebrake because of his ambitiousness.

(Note: English translation is not included in the Sama Bangingi primer.)

REFERENCES

- Ashley, S. Notes on Tausug orthography. In G. Rixhon (Ed.) Sulu Studies 2. Philippines: Coordinated Investigation of Sulu Culture, 1973.
- Carr, L. J. Situational analysis. New York: Harper & Brothers, 1948.
- Gray, W. S. The teaching of reading and writing. Chicago: UNESCO, Scott Foresman & Co., 1956.
- Johnston, E. Some psycholinguistic aspects of Brou literacy problems. Unpublished master's thesis, University of North Dakota, 1967.
- Modiano, N. National or mother language in beginning reading: a comparative study. Research in the Teaching of English, 1968, 2, 32-43.
- Morren, R. C. A descriptive study of the procedures involved in developing literacy materials for the Sama Bangingi: a preliterate society in the Republic of the Philippines. Unpublished doctoral dissertation, New Mexico: New Mexico State University, 1977.
- Neijs, K. Literacy primers and construction evaluation and use. Belgium: Imprimerie Casterman, 1961.

TESTING LINGUISTIC AND FUNCTIONAL COMPETENCE
IN IMMERSION PROGRAMS

K. Connors, N. Ménard, R. Singh, with the collaboration of
D. Beauregard, R. Lazure, J. Morse-Chevrier
Université de Montréal

I - Introduction : Immersion and immersion evaluation

For the last ten years, immersion programs have been greatly increasing in popularity as a means of teaching school children a second language, and in particular, of teaching English-speaking Canadian children French. In this context, "immersion" means the situation in which the student takes some or all of his courses in the second language. Along with the increase in the number of French immersion programs, particularly (but not exclusively) in Greater Montreal, has come an increasing number of immersion evaluations. These studies have sought to find out how much and how correctly the Anglophone subjects of immersion experiments are learning French, and have tried to settle other issues as well. These include the question whether the children conserve (and continue to acquire) native ability in their mother tongue, whether they are learning the subjects taught in the second language as well as their peers who are being taught the same subjects in their mother tongue, and a host of other cognitive and social-psychological questions which differ somewhat from study to study.

The present authors have been conducting an immersion evaluation study for a school board located in the area of Greater Montreal called the "West Island" and consisting of the suburbs from Dorval on the east to Ste-Anne de Bellevue on the west. The main subject sample consisted of 180 children, chosen by a systematic randomizing procedure, from those six schools in the district which contained immersion classes. Three grade levels were represented : grade 6, which is the first (and only complete) immersion year in this particular program, grade 7 (the first year of high school), and grade 9. In these early high school years, the students could take a program of "partial" immersion, which meant three courses in French.

Since our evaluation was intended to answer a number of diverse questions (only some of which interest us here), the study consisted of a rather long series of observations, tests, and analyses :

1. approximately seventy preliminary taped interviews at the beginning of the school year (before the random sample was designated), intended to suggest specific syntactic problems

on which a series of tests would focus.

2. a series of eight short, written tests of French syntax, each focusing on a specific type of structure. These were pre-tested, tested on the sample, retested on the same sample five months later, and tested on native Francophone and non-immersion Anglophone control groups.
3. a test of French vocabulary, focusing on lexical items current in the subjects' textbooks and in Quebec French generally. This test was also pre-tested, administered and re-administered to the main sample, and given to the Francophone and Anglophone control groups. In both (2) and (3), orthography was a factor in correction only insofar as it reflected a crucial distinction. Qui could not be written as que, for example; on the other hand, in vocabulary items and in verb forms, if what the student wrote reflected native pronunciation, it was accepted.
4. two standardized tests of foreign language aptitude, whose purpose and results unfortunately go beyond the scope of this paper.
5. a partially standardized questionnaire of attitude towards learning French (for Anglophone Canadian children). This also unfortunately cannot be discussed here.
6. a four-part standardized test of skills in French (listening, speaking, reading, and writing)--the "Pimsleur French Proficiency Tests".
7. a standardized test of written English skills, and
8. a standardized test of mathematical skills, neither of which concerns us here.
9. a syntactic and lexical analysis of samples from a sub-set of the students textbooks, passages chosen by a systematic randomizing procedure.
10. a syntactic and lexical analysis of a set of compositions written by the main sample subjects, from which passages were chosen by a procedure similar to that in (9). Topics were assigned to induce some students to write in past, some in present, and some in future tense forms.
11. approximately 30 observations and recordings of immersion classes, representing all the schools and grade levels in the study, and all the subject matters taught in French.
12. a questionnaire collecting limited socio-linguistic and socio-economic data from the sample, which need not concern us in detail here.

13. a series of short, taped interviews with a sub-set of the main sample in one school at the end of the school year.

II - Global judgments vs. structured tests

Like many other immersion evaluations, our study shows that immersion pupils learn significantly more French than their Anglophone peers outside immersion classes (according to the test of comparison of means - see appendix), who in Quebec English-language schools study French simply as a foreign language, normally from first grade (if not kindergarten) on. It also shows, however, that there is a statistically significant difference (comparison of means - appendix) between the Anglophones in immersion and their Francophone controls, even in the structured syntactic and lexical tests which all three groups underwent. But the differences between the Francophone controls and the "immersed" Anglophones on structured tests, significant though they are in this and other studies, fail to capture the vast differences in linguistic behavior between immersion Anglophones and Francophones of the same age. In our subjects, these differences were immediately evident to speakers of French in situations where pupils were called upon to use the language in utterances not completely structured in advance by the teacher or questioner. In general, Francophone judges do not take immersion pupils for Francophones (see also Lambert and Tucker, 1972, p. 68). Early immersion studies, in connection with the well-known St-Lambert experiment, called upon native speakers to judge the oral performance of immersion pupils (Lambert and Tucker, 1972). These solicitations of global judgments were couched in relatively non-technical terms such as the request to judge the speaker's grammar, pronunciation, and overall fluency on a 6-point scale. On these measures, Lambert and Tucker do find some significant differences in favor of the French controls (e.g., p. 40, p. 88, pp. 120-2, p. 148). But one gets the impression, when not told explicitly (p. 141), that there were few syntactic errors. Spilka (1976), however, reports on an error analysis based on a story retelling task given to 20 immersion pupils and 20 native controls (again the St-Lambert experiment). The quantified part of the study shows big differences between the immersion and Francophone pupils, in favor of the latter. The error-types counted were "incomplete sentences", "incorrect sentences", "gender errors", "preposition errors", "verb errors", "non-reflexive pronoun errors", and "reflexive pronoun errors" (pp. 149-50).

The structured syntactic tests with which we experimented in the present study were attempts to find specific correlates of the more global judgments which speakers or linguists had made in earlier studies. In what specific respects is the grammar of an immersion pupil deviant from a Francophone grammar, since we already know it is deviant? Is there one particular

structure on which the performance of immersion students correlates so highly with all other measures, including global ones, that we can regard it as an index of second language competence?

The eight syntactic tests with which this study experimented dealt with negation, interrogative structures, verb morphology, pronominalization, clitic placement, faire causatives, relativization, and article usage (see syntax sub-tests 1-8, appendix). We found that, among these sub-tests, the best index of global performance was, surprisingly, that on verb morphology (cf. Oller and Redding, 1971). It had the greatest number of correlations at the .001 level of significance or better with the other French proficiency sub-tests administered at the end of the school year : that is, the 4 Pimsleur French proficiency tests, the 3 sub-parts of our lexical post-test, and the other 7 syntactic retests.

In addition, we found that the comparison of performance on the syntactic pre- and post-tests distinguishes somewhat between grammatical structures which improve, in the given immersion situation, over five months, and those which do not improve significantly in that situation, over that period. This provides some evidence on the comparative psychological complexity of the different structures tested. What we have found is that there were at least 2 syntactic sub-tests which failed, in 2 out of 3 sample grade levels, to improve at the .05 level of significance or better : verb morphology and relativization.

We have also begun the task of comparing the errors made in the pre-test and post-test for various syntactic sub-tests. In a sub-sample of 20 pupils, the types of errors made on both sub-tests concerning pronouns (pronominalization and clitic placement—subtests 4-5 in appendix, analyzed together) did not change. In addition, the frequency order of no given error type changed by more than one rank. At least three syntactic sub-tests, however, did improve significantly in all main sample grade levels : negation, pronominalization, and questions. An analysis of the test and retest of all students (140) in the sample who took both syntax text 2 and syntax retest 2—interrogative structures shows the kinds of progress made, which here reduces to about half a dozen items of learning. The greatest surprise is that the most frequent single improvement by far is the identification (in the indirect question) of the interrogative word. It would make sense to do for French what some studies have begun to do for English--comparisons of first and second-language stages in the learning of specific structures (cf. Ravem, 1974).

III - Linguistic vs. functional competence

At best, such measures as those we have discussed up to now

are indices of linguistic competence. As we have mentioned, our subjects fall short of native performance even on these measures. Even significant differences on such measures, however, fail to do justice to the dramatic inadequacy of our immersion subjects in situations less structured than classroom recitation (cf. Lambert and Tucker, pp. 88-9). Both series of semi-structured interviews, one at the beginning of the school year, and one at the end, indicate this most forcefully. The overwhelming majority of the students interviewed were unable to carry on anything resembling a conversation and answered the interviewer's questions in utterances ranging from a monosyllable to a phrase (in the technical sense). The fact, then, that our immersion subjects demonstrably have considerable knowledge of linguistic structures but an inability to use them in relatively unstructured situations leads us to distinguish their linguistic from their functional or communicative competence. The former term is meant in its traditional Chomskyan sense. The latter, in our usage (see also Hymes, 1974) presumes linguistic competence and refers to the additional ability to use known structures appropriately. What is lacking in immersion evaluation research to date are adequate, detailed characterizations of both the linguistic and the functional competence of immersion pupils. We think that specific-structure-oriented tests of the kind we have experimented with may help solve the first of these problems. But the second, the detailed characterization of the functional inadequacy of immersion pupils' language, remains, here as elsewhere undone. The remainder of this paper seeks to account for the relative linguistic and especially functional incompetence of our subjects.

IV - Constraints on linguistic input :

The fact that neither the purely linguistic competence nor the functional competence of our subjects is near-native calls for an explanation. We believe that it lies in the skewed nature of the linguistic input our subjects receive and of the linguistic output they produce. The non-native nature of their linguistic and functional competence, in other words, is a function of the sort of language they are exposed to and the sort of language their extremely limited tasks require them to produce.

That the nature of linguistic input crucially determines the nature of the grammar constructed by the learner is well-documented (Labov, 1970). Crucial deficiencies in second-language input have, however, not been studied. It has been pointed out before that the data a second-language learner is exposed to is underdetermined in ways that may be systematically different from the ways in which the L_1 learner receives his linguistic input. But the nature of the input in second-language learning in general and immersion learning in particular has not been systematically examined.

As one way to study the linguistic input our subjects receive, we have examined a sub-set of their textbooks. Samples from these books have been coded for their lexical variety and syntactic complexity. Although textbook language is almost by definition a special, if not artificial language, we believe that it forms the basis for an unusually artificial linguistic input in this case, in so far as the program uses "home-made" texts and teaching aids designed specifically for the immersion student (cf. Lambert and Tucker, 1972, p. 26). Whereas the textbooks in subject areas such as math and art are ones intended for Francophones, some of the textbook and related material in social studies is of this special character. In addition, our subjects use a French-as-a-second-language text in French class.

The nature of the in-school linguistic input is also a function of the nativeness of the teachers. It is a fact that the program we evaluated employed a large number of non-native teachers. Although their academic credentials, professional competence, and adequacy in French are not open to question, it is well-known that non-natives generally use a more conservative and stylistically underdifferentiated grammar. The linguistic input provided by these teachers may also constrain the grammar constructed by the immersion students.

The differences resulting from the nature of the input received from teachers, texts, and other materials are, however, not as important as the differences resulting from the absence of certain kinds of linguistic input for these immersion subjects in their ordinary social circumstances. Once in high school, they rarely talk to each other in French, thus depriving each other of the chance to verify their tentative hypotheses about the grammar they are supposed to be learning (See Lambert and Tucker, 1972, p. 187 in this point). Even if our subjects could be persuaded to talk to each other in French beyond grade 6, the input provided in this way would, needless to say, be deviant.

The difference in the linguistic input, and the absence of certain kinds of input, is further exaggerated by the general sociolinguistic milieu our subjects are a part of. Most of them live in French-in-certain-classes-and-English-elsewhere universe (as our socio-linguistic questionnaire documents), so the input they receive is limited to the restricted non-peer code they are exposed to in school.

V - Constraints on linguistic output :

The restricted nature of the students' linguistic input is confirmed by the deviant and painfully simple nature of their output. Although lack of redundancy is considered a stylistic virtue, it goes to ungrammatical and socio-linguistically unacceptable lengths in our subjects' utterances in class and in

interviews.

In the light of these observations, what is surprising is that immersion students in other studies tend to approach (and occasionally to surpass) native performance on certain highly structured tests, especially when passive skills are concerned (e.g. Lambert and Tucker, 1972, p. 86). Lambert and Tucker show this repeatedly in their samples from what is, we must bear in mind, a different type of immersion program, using such measures as the "Test de Rendement en Français" of the Montreal Catholic School Board (see, p. 39, p. 59, p. 78, p. 94, p. 111, p. 134, p. 146). We submit that such highly structured "objective" tests of the vocabulary and orthographic norms of the written language tell us very little about approximation to native speaker competence.

Immersion subjects, as has been shown by Selinker, Swain, and Dumas (1975), employ non-native strategies. These strategies may result from the fact that most immersion programs are designed for children who are beyond the early stages of native acquisition or from the immersion student's attempts to cope with specific kinds of deficiencies in the linguistic input he receives and the output he has occasion to produce. Which is the case may well be a very difficult question to answer. It seems likely, however, that L_2 approaches L_1 acquisition, even before the critical age, only if the subject has access to the same sort of input a native receives. The important thing, of course, is not the quantity of exposure (see Krashen, 1973), but the linguistic normalcy and diversity of the input the subject receives and of the output activities he engages in.

It is true that the quantity of exposure to and use of French in an immersion program is considerably greater than in an ordinary second language program. This quantitative factor alone may account for the fact that, in evaluation studies, immersion subjects always show a greater command of French than non-immersion control subjects. But if the increase in quantity is not accompanied by situational and stylistic variety, it may not lead to anything like native linguistic, and especially socio-linguistic behavior.

We think the fundamental limitation on what immersion can do arises from its restriction to the classroom. From interviews with many of our subjects, from our socio-linguistic questionnaire, and from taped classroom observations of student interaction, it is clear that, particularly in the case of the high school students (cf. Lambert and Tucker, 1972, pp. 179-94), "real life" takes place entirely in English. As long as that continues to be the case in the social milieu our subjects represent, there is very little hope that their learning of French will approach acquisition.

REFERENCES

- Hymes, Dell. 1974. Foundations in Sociolinguistics : an ethnographic approach. Philadelphia : University of Pennsylvania Press.
- Krashen, Stephen D. 1973. "Two studies in adult second language learning". Paper read at Winter meeting of the Linguistic Society of America.
- Labov, William. 1970. "The study of language in its social context", Studium Generale 23, pp. 30-87.
- Lambert, Wallace E. and G. Richard Tucker. 1972. Bilingual Education of Children. Rowley, Mass. : Newbury House.
- Oller, John W., Jr. and Elcho Z. Redding. 1971. "Article usage and other language skills", Language Learning 21, pp. 85-95.
- Ravem, Roar. 1974. "The development of Wh-Questions in first and second language learners," in Richards, Jack C. (editor). Error Analysis : Perspectives on Second Language Acquisition. London : Longman. Pp. 134-55.
- Selinker, Larry, Merrill Swain, and Guy Dumas. 1975. "The interlanguage hypothesis extended to children", Language Learning 25 : 1, pp. 139-152.
- Spilka, Irène V. 1974. "Assessment of second-language performance in immersion programs", Canadian Modern Language Review 32 : 5, pp. 543-61.

APPENDIX

REMARKS ON THE STATISTICAL DATA

- a) The index z is a standard scale for comparison of means. When $z \geq 2$, this is a significant difference (= s.d.) at the .05 level of significance. When $z \geq 3$, it is at the .003 level. m = mean; var = variance, which indicates how close the individual scores in the sample or group are to the mean.

- b) Number of pupils :

		<u>SAMPLE</u>	<u>ENGLISH CONTROLS</u>	<u>FRENCH CONTROLS</u>
Sec I	Syntax	50	30	24
	Lexicon	49	30	24
Sec III	Syntax	56	20	20
	Lexicon	52	20	20

- c) In certain cases, when one of the two variables does not follow a normal distribution curve, it is not advisable to make the comparison using the index z . This is why we have left certain boxes empty.
- d) In the tests and sub-tests, the comparison of means leaves no doubt about the superiority of the sample to the English Control group, neither in Secondary I nor in Secondary III. The French Control group is also clearly superior to the sample.
- e) The maximum number of points possible in each test and sub-test appears in parentheses.
- f) The use of the index "t" of Student-Fisher, which is more conservative, would not change the rejection of the hypothesis of random distribution.

SECONDARY I

COMPARISON OF MEANS

RETESTS	SAMPLE	ENGLISH CONTROL GR.	FRENCH CONTROL GR.	SAMPLE vs ENGL. GR.	SAMPLE vs FR. GR.
(105 pts) m syntax var	60.11 318.97	22.48 171.92	89.36 156.30	z = 10.60 s. d.	z = 8.00 s. d.
(40 pts) m lexicon var	19.67 24.64	13.93 19.72	34.13 13.85	z = 5.26 s. d.	z = 13.77 s. d.
SUBTESTS					
(8 pts) m syntax 1 var	5.58 3.96	2.70 3.39	7.63 0.42	z = 6.54 s. d.	z = 20 s. d.
(11 pts) m syntax 2 var	9.04 6.49	4.13 14.95	11.00 0.00	z = 6.13 s. d.	
(18 pts) m syntax 3 var	10.70 28.30	2.87 10.53	16.08 8.34	z = 8.32 s. d.	z = 5.72 s. d.
(13 pts) m syntax 4 var	5.26 7.09	1.87 2.33	10.54 11.39	z = 7.23 s. d.	z = 6.58 s. d.
(5 pts) m syntax 5 var	1.86 1.84	0.88 0.56	3.75 1.15		
(7 pts) m syntax 6 var	3.34 6.35	0.33 0.92	6.17 2.75		
(4 pts) m syntax 7 var	2.80 1.71	0.73 1.31	3.63 0.85		
(39 pts) m syntax 8 var	27.12 46.82	15.00 52.62	37.21 7.82	z = 7.26 s. d.	z = 8.78 s. d.
(12 pts) m lex 1 var	7.80 3.58	5.90 2.09	11.00 1.22	z = 5.08 s. d.	z = 9.23 s. d.
(10 pts) m lex 2 var	3.78 2.72	3.30 4.08	8.33 2.84	z = 1.07 non-s.d.	z = 10.72 s. d.
(18 pts) m lex 3 var	8.10 7.39	4.73 5.31	14.79 3.39	z = 5.91 s. d.	z = 12.21 s. d.

SECONDARY III

COMPARISON OF MEANS

RETESTS	SAMPLE	ENGLISH CONTROL GR.	FRENCH CONTROL GR.	SAMPLE vs ENGL. C. GR.	SAMPLE vs FRENCH C. GR.
(105 pts) m syntax var	74.76 195.81	24.95 220.84	90.59 73.98	z = 14.88 s. d.	z = 5.84 s. d.
(40 pts) m lexicon var	24.19 18.71	11.80 12.27	33.85 8.66	z = 12.27 s. d.	z = 10.61 s. d.
SUBTESTS					
(8 pts) m syntax 1 var	6.91 1.21	2.75 3.78	7.65 0.45	z = 9.04 s. d.	z = 3.70 s. d.
(11 pts) m syntax 2 var	10.07 1.96	4.35 14.77	10.95 0.05	z = 6.35 s. d.	z = 4.40 s. d.
(18 pts) m syntax 3 var	14.30 16.18	4.95 17.10	15.90 9.36	z = 8.57 s. d.	z = 1.79 non-s. d.
(13 pts) m syntax 4 var	7.50 10.44	2.35 3.71	11.00 2.95	z = 8.31 s. d.	z = 5.93 s. d.
(5 pts) m syntax 5 var	2.95 1.43	0.90 0.62	3.85 0.66		
(7 pts) m syntax 6 var	4.38 6.57	0.30 0.64	6.00 3.90		
(4 pts) m syntax 7 var	3.25 0.81	0.75 1.36	3.90 0.10		
(39 pts) m syntax 8 var	31.00 24.55	15.25 48.41	37.60 2.99	z = 9.09 s. d.	z = 8.45 s. d.
(12 pts) m lex 1 var	8.13 3.92	4.80 2.48	11.35 0.77	z = 7.26 s. d.	z = 9.29 s. d.
(10 pts) m lex 2 var	5.37 4.51	2.55 2.79	8.20 1.54	z = 5.76 s. d.	z = 6.86 s. d.
(18 pts) m lex 3 var	10.69 4.77	4.45 2.26	14.30 4.12	z = 13.57 s. d.	z = 6.48 s. d.

POUR UNE PRISE DE CONSCIENCE
DES HABITUDES ARTICULATOIRES MATERNELLES
DANS L'ETUDE D'UNE LANGUE SECONDE

Joan Neuburger Savitt

SUNY Albany

[This paper argues that high school or university students undertaking the study of a second language should begin such study by becoming aware of their own basic native articulatory habits. An awareness of their own ingrained patterns can help them to sharpen their ability to learn the corresponding habits employed by speakers of the target language. A series of model exercises is then presented which can be used in awareness training for anglophones beginning the study of French.]

Si tout étudiant d'une langue seconde était bon imitateur, la nécessité d'une prise de conscience de ses habitudes articulatoires maternelles ne se présenterait pas. Mais en réalité la plupart des étudiants ne peuvent pas reproduire une phrase donnée avec une prononciation et une intonation correctes, étant trop restreints par leurs habitudes articulatoires inconscientes. Les rares étudiants capables d'imiter une phrase au début de leurs études profiteront quand même d'une explication technique de ce que leur faculté singulière leur permet de faire.

La plupart d'entre nous avons à combattre nos habitudes fixes au début et au cours de nos études. Les étudiants avancés ont souvent besoin d'être rééduqués à cause de leur manque de préparation phonétique. Le propos de ma thèse est de soutenir que nous pouvons améliorer la situation pénible de nos étudiants avancés futurs grâce à un changement de nos méthodes d'introduction employées avec les débutants, surtout si ce début d'études se fait au lycée ou à l'université, c'est-à-dire, au moment où les étudiants ont perdu en partie leur flexibilité linguistique, mais aussi au moment où on peut introduire un sujet technique pour les aider dans leur apprentissage d'une langue seconde.

Ici, pour illustrer cette thèse, je traiterai de l'apprentissage du français par des locuteurs anglophones. Quelles que soient les langues envisagées, même si les détails changent, ce programme pourra toujours s'adapter.

A mon avis, le grand défaut de notre enseignement de la prononciation française aux étudiants anglophones

est de négliger leur connaissance profonde mais inconsciente de l'anglais. Il est possible d'amener l'étudiant à se rendre compte lui-même de ses habitudes inconscientes. Quelles habitudes alors faut-il souligner? Pour les débutants, on ne peut que développer les articulations et les connaissances essentielles, c'est-à-dire, celles de l'accentuation du mot et de la phrase, la structure syllabique et le rythme des syllabes, et la position articulatoire de la bouche au repos, cette dernière pour introduire l'inventaire phonémique second.

La liste que je viens de dresser comprend seulement certaines habitudes articulatoires de base. Mais la liste est courte aussi parce que j'envisage un programme qui répondra aux besoins spéciaux des débutants. Les habitudes à adopter que j'ai choisi d'introduire au début sont, naturellement, celles qui sont en conflit avec les habitudes anglophones correspondantes. En même temps, ce sont des habitudes qui sont faciles à souligner. Donc, pour des raisons pédagogiques, l'intonation est un sujet que j'ai choisi de laisser aux pouvoirs d'imitation des étudiants, bien qu'elle soit aussi importante que les autres facteurs que je choisirai de présenter, et je la réserve pour une autre leçon.

Un programme d'introduction n'est pas un programme de correction. Comme enseignants de langue, nous avons à notre disposition maintes techniques de correction pour les étudiants avancés qui sont malheureusement prisonniers de leurs mauvaises habitudes renforcées. Les étudiants qui sont depuis longtemps engagés dans leurs études françaises ont besoin de ce genre de correction. Par contre, les exercices que je proposerai sont destinés à entraîner les débutants à des habitudes correctes dès le début de leurs études et ne sont correctifs que dans ce sens.

Les exercices pour les débutants ont en général deux buts: l'un est d'établir la nature des habitudes anglophones et l'autre d'introduire les habitudes francophones correspondantes. Toujours, le point de départ de ces exercices se trouve dans la langue maternelle de l'étudiant, puisqu'elle est celle qu'il connaît.

Considérons comme premier exemple de cette méthode l'introduction de l'accent démarcatif du mot français. Pour signaler aux étudiants la nature de leurs habitudes déjà acquises et inconscientes, on demande d'abord à l'étudiant de trouver la syllabe la plus forte dans chaque mot d'une liste donnée: [N.B. Pour chaque exercice, j'indiquerai un exemple modèle avec les réponses désirées.]

- (1) Underline the strongest or loudest syllable in each word. EX: comfortable
happy, syllable, creative, ignition,

thunder, enthusiastic, distress,
nation, radio, orange, television

Cet exercice peut être préparé par l'étudiant à l'avance et hors de classe. Un tel exercice rappelle à l'étudiant que l'accent peut tomber sur n'importe quelle syllabe d'un mot anglais, mais que l'accent est fixe sur telle ou telle syllabe pour un mot donné, ce qui permet, par exemple, de distinguer le nom 'reject' du verbe 'reject'. L'étudiant remarquera que si l'accentuation finale est rare, elle est quand même possible. Alors on demande à l'étudiant de chercher des mots anglais à accentuation finale et de préparer une liste de dix mots:

- (2) Find ten English words stressed on their last syllable. Check your list by testing the stress on each syllable of the word.

EX: 'reject' 'important'
accept 'report'

confer, repeat, referee, jamboree,
behead, congeal, tambourine,
discuss, reply, behold

Cet exercice aide l'étudiant à acquérir l'accent français correct puisque l'étudiant doit vérifier chaque mot qu'il propose en mettant l'accent sur chaque syllabe. En se trompant ainsi, il apprend.

Les étudiants ont alors deux listes. La première comprend des mots à accentuations diverses, et la deuxième des mots seulement à accentuation finale (au moins on l'espère). Le lendemain, en classe, on fait lire la deuxième liste de mots, c'est-à-dire, la liste qu'a préparée l'étudiant lui-même. On explique alors que l'accent français est toujours fixe sur la dernière syllabe du mot. Pour faire le contraste entre le français et l'anglais, on passe à la présentation des mots analogues, en insistant sur les différences de position:

- (3) Compare the position of the stressed syllables in these cognate words as you hear them read aloud.

radio, imitation, apartment (appartement),
government (gouvernement) television

Pour transférer la conscience de l'accent démarcatif du mot à celui de la phrase, on a de nouveau recours à l'anglais. On demande cette fois à l'étudiant de trouver les parties les plus fortes d'une phrase entière:

- (4) Find the stressed word or words in each sentence.

A 1: This is apple jelly, and this is
pear jelly.

A 2: This is apple jelly, and this is apple jam.

B 1: It isn't quarter after two; it's quarter of two.

B 2: It isn't quarter after two; it's quarter after three.

Ces couples de phrases montrent que l'accent anglais change de place si la nature de la question ou de la comparaison change. La deuxième phrase de chaque couple s'approche du modèle français et peut servir comme phrase de transition dans le travail suivant. Notez qu'il faut signaler qu'une telle accentuation dans une phrase française n'est pas une accentuation comparative, mais simplement une accentuation neutre.

Passons à la présentation de la structure syllabique française. La chose la plus importante à signaler ici est la syllabation ouverte du français. Prenant comme point de départ habituel le langage de l'étudiant, on demande d'abord à celui-ci de compter les syllabes dans une liste de plusieurs mots:

(5) Count the syllables in each word.

EX: sister: 2.

one, hello, resourceful, irregular,
extraordinary (1,2,3,4,5)

Les étudiants trouveront cet exercice facile. Alors on passe à un travail plus difficile: on leur demande de trouver les limites syllabiques. D'abord on prend des mots assez faciles:

(6) Pronounce each word in separate syllables to find the syllable boundaries. EX: in-vite
repeat, lightswitch, table

Une deuxième liste de mots permet de révéler la vraie nature de la syllabation anglaise:

(7) Can you find clear syllable boundaries in these words?
repetition, lightning, lighter,
tabular, respiration, elevate

En anglais, à vrai dire, les limites syllabiques claires n'existent pas dans la plupart des cas à l'intérieur des mots polysyllabiques. Ceci remarqué, on présente le fait qu'en français les limites syllabiques sont nettes, et qu'en principe toute syllabe doit commencer avec une consonne et se terminer par une voyelle, ce qui donne, en cours de locution, une bouche ouverte. Par contre, les syllabes anglaises se terminent généralement avec une bouche fermée à cause de la présence d'une consonne finale non-relâchée. Pour travailler ce point, on demande aux étudiants d'articuler quelques

mots anglais selon la pratique française:

- (8) Divide these words into syllables the way a French speaker would by beginning each with a consonant and ending each with a vowel where possible. EX: re-pe-ti-tious
respiration, really, very, any, kick,
eventually, different, divisional

Si, en faisant ce dernier exercice, les étudiants mettent soigneusement l'accent sur la dernière syllabe de chaque mot, on peut mettre en évidence la relation qui existe entre la syllabation ouverte, l'accent démarcatif, et la durée des syllabes. Pour ce faire, il faut démontrer qu'en anglais, plus l'accent est initial, plus les syllabes suivantes sont prononcées rapidement et téléscopées, mais que, par contre, au moins avec les mots bisyllabiques, si l'accent se trouve sur la deuxième syllabe, les syllabes sont de durée à peu près égale:

- (9) Tap out the rhythm of the word and number sequences given:

deeper, even, eventful, eventually;
(waltz time) 1,2,3.

Compare their rhythm to that of:
event, depend, refer, comply;
(count) 1,2,3; 1,2,3,4,5

Cet exercice met en relief l'effet de la position de l'accent sur le rythme. Alors on introduit le concept de l'égalité de durée des syllabes françaises.

Ces exercices terminés, où en sont nos étudiants maintenant dans leur connaissance de la phonétique française? Ils comprennent la nature de l'accentuation et de la syllabation françaises, mais ils ne peuvent rien prononcer. C'est alors le moment d'introduire les sons du français eux-mêmes. Mais la présentation des phonèmes doit aussi être préparée. Avant de faire répéter des sons particuliers, il faut comparer la position au repos de la 'bouche anglophone' à celle de la 'bouche francophone'.

Un anglophone qui ne parle pas en général garde sa langue dans la position montrée dans le dessin (10):

(10) anglophone



(11) francophone



La pointe de la langue touche les alvéoles; la langue est plate et se tient contre le palais. Les muscles des joues ne sont pas contractés. Une fois la bouche ouverte, le son qui sortira sera [ə]. Par contre, la

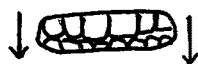
bouche francophone à l'état de repos peut avoir la position indiquée dans le dessin (11). La pointe de la langue reste derrière les dents inférieures, le dos de la langue est très avancé, et les muscles des joues sont légèrement contractés. Tout ceci donne la voyelle neutre [ə] si la bouche est ouverte sans aucun changement.

Cette explication de la position de repos risque peut-être d'offenser le linguiste, mais dans la pratique il est très utile de dire à l'étudiant anglophone de garder la pointe de sa langue contre les dents inférieures, de faire toucher les bords de la langue contre les dents canines supérieures, et d'essayer de maintenir une tension musculaire dans ses joues.

La tension musculaire donne une voix aiguë par nature. Pour introduire cette tension musculaire dans la bouche, on a recours à plusieurs techniques que j'ai trouvées efficaces. Pour une tension générale, on peut demander à l'étudiant de baisser un tout petit peu la mâchoire tout en gardant les lèvres fermées. Et pour joindre plus tard cette tension musculaire à une position des lèvres soit écartée, soit arrondie, il existe deux autres techniques. Pour les voyelles arrondies, l'étudiant s' imagine qu'il tire un sucre d'orge de sa bouche tout en essayant de la retenir avec sa langue et ses lèvres (voir 12). Pour les voyelles écartées, l'élève fait un geste qui découvre toutes ses dents inférieures en faisant un rectangle de sa bouche (voir 13). Ce dernier geste demande l'emploi des muscles que les anglophones n'emploient point d'habitude. Il est très efficace, et, pour les débutants, fatigant aussi, mais il est beaucoup plus utile que le sourire que l'on suggère ordinairement, parce que le sourire utilise des muscles différents. (A mon avis, ce dernier geste explique aussi le visage caractéristique des francophones.)

(12) arrondie antérieure

(13) écartée



Cette série d'exercices terminée, on passe à l'écoute et à l'imitation de l'inventaire phonémique lui-même pour indiquer aux étudiants les possibilités phonétiques auxquelles ils auront affaire. Pour ne pas trop frustrer les étudiants dans leur désir d'apprendre à 'dire quelque chose', cette présentation peut se faire en conjonction avec des mots communs tels que les chiffres 'un' à 'dix' et certaines expressions utiles telles que: "Bonjour! Comment vas-tu? Bien, et toi? Moi aussi." Il suffit ici de présenter les voyelles et leurs expressions graphémiques communes; pour les

anglophones, au début, les consonnes sont assez faciles à distinguer. Et ainsi se termine l'initiation phonétique propre. Les étudiants sont depuis ce moment lancés dans l'étude de la langue française.

La totalité de cette introduction se fait en une heure si les étudiants préparent les deux premiers exercices à l'avance. Donc, le programme est vite terminé. Mais l'entraînement linguistique que les étudiants auront reçu pendant leurs premières séances les aidera à reproduire et à se représenter ce nouveau langage. Cette acquisition se fera de telle façon que leurs premiers pas en français seront suffisamment assurés pour ne pas avoir besoin de correction par la suite.

¹ Peut-être mieux exprimé "la position articulatoire préalable à l'énonciation d'une phrase dans une langue" que "la position de la bouche au repos". La notion de l'existence d'une telle position m'a été suggérée par Edgar N. Mayer en 1968.

IV

SOCIOLINGUISTICS: SOCIAL VARIABLES INFLUENCING LANGUAGE USE

THE LANGUAGE OF MALE-FEMALE HUMOR IN POPULAR PULP

Beth Bethel and Bettie Horne

Lander College

The purpose of this study is to present an analysis of both the linguistic and nonlinguistic elements of cartooning in Playboy and Ladies' Home Journal. Such an analysis will serve to identify the differential ways in which humor is structured for primarily male and primarily female audiences. The analysis will be conducted with the context of mass communication theory, which has traditionally focused on the persuasive character of media; here media are recast into the role of information transmitters. In assuming that the implicit nature of humor is a reconciliation of contradictions inherent in social life, the cartoons are examined as representative of an information form particularly salient to the male and female spheres of social life. From this perspective cartooning is viewed as a most suitable modality for identifying those elements of the cultural system which are uniquely male or uniquely female.

Humor, Gender, and Popular Pulp

In proposing to examine cartooning as it portrays male and female aspect of social life it is assumed that this particular form of abbreviated humor has a significant place within the cultural milieu. A "bewildering diversity of theories"¹ attempting to explain the nature of humor have been set forth at various points, ranging from Plato to Hobbes to Freud. They have in common an assumption that inherent psychological weaknesses can be viciously expunged through the deprecation of others, within the context of humor.

Experimental psychology has offered a tentative but empirically derived explanation of the nature of humor in the tension reduction hypothesis² and its variation, the catharsis/aggression hypothesis.³ Eysenck's⁴ stress of incongruity and contradiction as central components of humor seems, however, more appropriate to an understanding of mass media cartooning, particularly in light of the tremendous popularity of the sometimes bawdy, sometimes piercingly satiric Yellow Kid of the 1890's newspaper fame.⁵ Certainly, as Hilbert suggests, humor, through cartooning, would allow one "reasonable way to make sense of the world" of irreconcilable contradictions.⁶

Eysenck and Hilbert, offering relief from the intellectual pitfalls of psychological reductionism, offer as well an explanation of the nature of humor in popular pulp. Cartooning becomes the reconciliation of contradictions inherent in social life. Because the material under question specifically addresses male and female audiences, it is assumed that the cartoons address, topically, those

areas of contradiction which are uniquely tied to male/female role performance and participation in social life.

In summary, the mass media, traditionally studied as agents of persuasion, are viewed here as transmitters of information; from this perspective the focal question of media research must be one which pursues an understanding of the structure of the information transmitted. Data from sociolinguistics provide the basis for hypothesizing that the content of male and female oriented media will typify gender-related concerns and will linguistically reflect real differences in men's and women's language. Within the cultural context, humor has been cast as a technique for reconciling contradictions between the ideal and the real. These contradictions are, like language and topic, assumed to be gender-related. Thus, media humor transmitted within gender-specific contexts should reflect gender-specific cultural concerns, linguistic as well as nonlinguistic. An analysis of such humor, as found in Playboy and Ladies' Home Journal, will be guided by two basic questions: (1) How do the cartoon topics address contradictions which are unique aspects of male and female participation in social life? (2) How does the linguistic stance provide the basis for distinguishing between male and female representations of their differential spheres of social life?

The Pulp: Is It Comparable? ⁷

Audience analysis data indicate, not unexpectedly, that the readership of Playboy is primarily male, and, likewise, that the readership of Ladies' Home Journal is primarily female. Age breakdowns of the two magazines reveal that both draw a sizable minority of their readership from the twenty-five to fifty year age group.

Audience Similarities of Playboy and Ladies' Home Journal

	<u>PB</u>	<u>LHJ</u>
readership in the 25-50 year age bracket	43%	48%
median income	\$15,200	\$14,859
professional/managerial occupations	23%	29%
attended college	38%	37%
married	44%	40% *

* This figure is probably deceptively low. An unknown percentage of the LHJ readership are older women who have been married but are now widowed.

Median age of readers (Playboy = 28.6 years; LHJ = 39.6 years) indicates that the Playboy readership draws heavily on the eighteen

to twenty-five year age group for its second major group of readers, while Ladies' Home Journal draws on the over-fifty year age group for its second major category. Both groups, however, seem to have their primary appeal to the middle age ranges. Median incomes indicate that the two readerships are drawn from similar socioeconomic categories and this is confirmed in occupational and educational similarities. These data provide the basis for assuming that it is not only reasonable but theoretically valid to compare the content of these two magazines. Their readerships are comparable in age, education, income, and marital status, the major demographic variables commonly used to establish sociological similarity between populations.

The Pictures: What Was Funny?

The quantity of cartoons appearing in the two magazines may be indicative of a particular demand of the audience. Three times as many cartoons appear in Playboy as opposed to Ladies' Home Journal. The categories of concern, perhaps not surprisingly, showed a concentration of interest in Playboy with sexual matters, vis a vis Ladies' Home Journal's more generalized subjects of domestic matters. If the category of purely sexual topics (over one-third of the Playboy cartoons) is discounted as a feature of audience demand, the two magazines were remarkably similar. Their mutual treatment of such common concerns as social exchange, family relationships, and finances is illustrated in the following table:

	<u>LHJ</u>	<u>Pb</u>
Love/marriage/affection	5	2
Money or business	7	28
Clothes, furnishings, etc.	5	4
Food, eating, drinking	6	7
Family Relations	4	23
Health	2	15
Legal Concerns	0	7
Sex	0	92
Other *	4	12

*Includes politics, education, religion.

An interesting observation is that Playboy reflected relatively more concern with matters of health than did Ladies' Home Journal, yet neither paid much attention to such areas as politics, educa-

tion, or religion. As might be expected, the "larger world" issues of crime, legal involvement, and sports appeared in Playboy and paralleled the Ladies' Home Journal preoccupation with domestic items such as food and clothing. Little regard was reflected in either magazine for the areas of professional behavior or ambition, a surprising feature considering the apparently middle-class makeup of the readership of both magazines.

If the setting of the cartoon is to be considered as a contributing factor in the message communicated to a reader, then it is interesting to note that, again, the bedroom, motel room and bar constituted the primary focus of the Playboy cartoons, as opposed to the living room and other non-sexually related rooms of the home in Ladies' Home Journal. Of note is the relatively large number of cartoons in both magazines that depicted the neutral setting of the outdoors.

In summary, Playboy humor can be characterized as primarily "Non-Domestic Sex" and the Ladies' Home Journal humor as "Non-Sexual Domesticity."

The Language: Who Talked?

If men are, as Henley and Thorne have suggested, linguistically dominant, then there are observable differences in the gross speech patterns of male and female cartoon figures. Based on a randomly selected sample of twenty cartoons from each magazine, several aspects of these potential differences were investigated. The first variable examined was overall length of the cartoon captions. No differences were found in the overall length of male-oriented and female-oriented messages between the magazines.⁸ In short, the magazine did not make a difference in the gross length of a message. However, the Playboy male characters had consistently longer messages than did the females. (The average message length for Playboy male figures was 14.45 words whereas the average message length for female figures was 11.00 words. This difference was a statistically significant one.) There was no difference between male and female characters' message length in Ladies' Home Journal. While the length of a message is only a gross estimation of dominance, these data provide the basis for an initial and tentative contention that Playboy humoring is portraying males in a dominant stance whereas the Ladies' Home Journal humor is tending more toward linguistic egalitarianism.

This initial analysis of message length considered only the sex of the cartoon speaker. It is possible that an analysis of the interaction between the sex of the speaker and the sex of the receiver will provide more explicit insight into this aspect of linguistic dominance. When the gender-related variables were considered in this way it was found that men tend to talk longer when they are talking⁹ to other men than when women are talking to either men or to women. This was the case for both magazines. The shortest message length was in those cartoons where women were talking to other women. This

would seem to destroy the stereotype of women as chronic and incessant talkers and replace it with an empirically derived assertion that one of the cues of dominance is overall length of a message, regardless of its content.

In addition to this overall analysis of message length the cartoon captions were also analyzed for more subtle differences between male and female characters which could also be interpreted as dominance cues. Male characters did not initiate or monopolize interactions more often than women. However, when they did speak they tended to address other men more often than women. This was also the case for female characters. Regardless of the sex of the speaker male receivers were more frequent than females.¹⁰

The Language: How Did They Say It?

Although there was a fairly equitable distribution in the numbers of male/female speakers in the 51 Ladies' Home Journal cartoons, in the 245 Playboy cartoons over twice as many males spoke as women, a fact which indicates the more aggressive pattern of male speech suggested by Lakoff. In the matter of speaker-receiver sex dichotomies, Ladies' Home Journal shows primarily opposite-sex members engaged in communication, although women addressed men more frequently than they addressed other females; while in Playboy one sees by far a greater number of male speakers, with most speaking to other males. In other words, men were the most popular choice as receiver for both sexes in both magazines.

One intriguing observation is that of the role of speaker as information provider or seeker. LHJ showed equal distribution of cross-sex encounters in providing information, with more women seeking information of men than vice versa. On the other hand, Playboy showed men providing information to other men most, and then next to women, and yet women in Playboy provided information only to men but not to other women. Both men and women sought information from men in Playboy.

If cartoons portray a generally abbreviated form of communication, this accounts for the preponderance in both magazines of the general absence of modifiers in all speech as well as the use of simple, declarative sentences. However, LHJ again showed a more balanced distribution of complexity in sentence structure, while Playboy showed significantly more complex structure in male speech only. Revealingly enough, Playboy depicted women as more than six times as likely to speak in simple sentences when addressing a man as opposed to a woman, while women addressed more questions and incomplete sentences to men than they did to women. Men in LHJ made more demands of others than did women, but they as often sought information from others. Women in Playboy were four times as likely to generate statements as opposed to commands when they addressed men, yet they freely directed commands to other women.

LHJ revealed both male/female speakers as generating sound, grammatically constructed sentences; whereas Playboy allowed to men a larger number of unfinished sentences and incomplete utterances than it allowed women.

Lexical features are another detail of speech which reflect not only the habits of speakers but the expectations of readers. Both magazines offered male speech marked by numerous instances of traits often indicative of submissive speech, including the use of interrupters, interjections, and nouns of direct address. Likewise, male speech was interrupted or tentative in more cases in both LHJ and Playboy, while LHJ offered no instances of tentative speech in females as defined by the use of ellipsis to indicate a lapse of time. Men in both magazines interspersed utterances with the use of names, titles, and interjections, with Playboy offering "baby," "God!," "Geez," and "damn," while LHJ tended toward the more restrained "golly," or "my god." Women in Playboy addressed men as "sir" and "chief," but no male refers to any female in either magazine as a superior or in a courteous manner, although men in Playboy do deferentially address one another as "Mister," "Sir," or "Gentlemen."

The Overall Effect: What Were the Emotions?

The two samples of cartoons were rated by Humanities and Social Sciences faculty at Lander College for intensity of emotional referents on six distinguishable emotions: anger, affection, fear, greed, envy, and lust. The raters were asked to rank each of the selected cartoons on a scale of intensity ranging from a low of one to a high of ten, selecting the emotion which they felt to be most appropriate to each cartoon. While these ratings reveal little about the general topical nature of the cartoons, or the types of interactions most prevalent in the cartoons, they are good general indices of the differential emotive stances assumed in male and female humor. Three questions can be pursued from the rating data: (1) Do emotional referents differ in male and female humor? (2) Are there identifiable clusterings of emotional referents which distinguish male and female humor? (3) Do the emotional referents vary in intensity for male and female humor?

The data demonstrated that there are two major emotional referents which tend to distinguish male and female humor, and in many ways these referents were consistent with the topical divisions between the two magazines noted earlier. "Lust" was listed as the major referent for the Playboy cartoons whereas "affection" emerged as the major referent for the LHJ sample. Anger and fear had equivalent frequencies of occurrence in the two samples; greed was not judged to be present in the Playboy sample nor was envy evident in the LHJ sample.¹¹

The two emotional referents "lust" and "affection" constitute differential emotional cores for male and female humor. From these two

basically different foci the analysis was extended to examine the composition of two potentially different humor constellations. These constellations were gauged through the emotional referents mentioned in conjunction with the two primary emotions. In the Playboy sample lust was the sole referent in thirty per cent of the cartoons examined. In combination with other referents, which included anger, envy, affection, and fear, it constituted an emotional referent in fifty per cent of the cartoons; but, no systematic relationship with other emotions emerged in this secondary analysis. Examination of the LHJ cartoons stands in contrast to this. While the Playboy data showed lust standing as an independent emotional referent in one-third of the cases affection was linked to anger in forty-four per cent of the total ratings. In only twenty-seven per cent of the ratings did affection emerge as the sole emotional referent. The central difference between the two samples, then, is not in terms of the emergency of a single emotional referent, but in terms of the consistent tendency for affection to be linked to anger in the LHJ cartoons.

When the ratings are examined for emotional intensity rather than for frequency, neither of the most frequently occurring emotions--lust and affection--emerge as the most intense. In the Playboy sample envy was the most intense emotion and in the LHJ sample the most intense emotion was greed. While the intensity ratings were not statistically significant in their differences, it is nonetheless somewhat incongruent that frequency and intensity did not correspond.¹²

Discussion

The initial research concern of this study was a delineation of the ways in which male and female humor differ, both linguistically and nonlinguistically, in magazines whose readerships are distinctly male and distinctly female. From the outset it was assumed that because of the gender-related differences in the two readerships the topics and concerns portrayed in the cartoon humor would vary; and that this variation would reflect the differences in male and female social concerns. This assumption was in part verified through an analysis of the topics which provided the basis for the cartoon humor. Predictably, male humor concentrated on sexual matters and female humor on domestic matters. Neither of the cartoon samples seemed to be concerned to any extent with more global matters such as contemporary political and economic issues, occupational problems, and the like.

In both magazines men as well as women have manifested lexical features which have been commonly described as "submissive" and associated with women's language. In both samples, however, men were preferred receivers of messages.

In Playboy women, when speaking to men, tend to employ direct and simple sentence structure to the exclusion of other alternatives.

When speaking to other women, however, Playboy women exhibited both balance and variety in their language to other women. LHJ women speakers, whether they addressed men or other women, offer more variety in the sentence structure.

An analysis of the emotional referents of the two cartoon samples, as established through objective ratings, revealed a plurality of emotions in the Playboy cartoons. Lust emerged consistently, but not in the role of the dominant emotion, despite the dominance of sexual matters in the Playboy topics. In the LHJ cartoons, on the other hand, the opposite was the case. There was a multiplicity of topics, but the primary emotional referent which emerged through the rating process was affection. This was consistently linked to a second emotion, anger.

The topic of language is only the superficial level of meaning. At a more subtle, yet more meaningful level, the form of the language speaks clearly of the position of the male/female when the role of speaker is assumed. On the one hand, the "woman's magazine" treats both men and women equitably in their language production. Playboy magazine, provides a stereotypical view of male speech as direct, complex and addressed to other males, but that of the woman as nonassertive, simple, and male-directed.

FOOTNOTES

¹ D. Berlyne. "Laughter, Humor, and Play." Handbook of Social Psychology, Vol. III, 2nd edition. G. Lindzey and E. Aronson, editors. (Reading, MA: Addison-Wesley, 1969) 795-852.

² D. Byrne. "Drive Level, Response to Humor, and Cartoon Sequence Effect." Psychological Reports, 4 (1958), 439-442.

³ D. Singer, "The Cathartic Function of Humor." Phd. D. Thesis, Yale University, 1964.

⁴ H. Eysenck. "Appreciation of Humor - An Experimental and Theoretical Study." British Journal of Psychology, 32 (1952), 295-309; H. Eysenck, Dimensions of Personality (London: Routledge, 1947)

⁵ M. DeFleur and S. Ball-Rokeach. Theories of Mass Communication, 3rd edition (New York: David McKay, 1975), 29.

⁶ R. Hilbert. "Approaching Reason's Edge: 'Nonsense' as the Final Solution to the Problem of Meaning." Sociological Inquiry, 47 (1977), 23-31.

⁷ We thank Dick Dignova of the Playboy Enterprise's Market Research Division and Barbara Schork of Downe Publishing's Marketing Department for their helpful cooperation in ferretting out these data on their readerships. Additionally, we note that LHJ has been publishing since 1883, Playboy since 1953.

⁸ A two x two analysis of variance of message length using sex of speaker and type of magazine as the independent variables yielded the results summarized in Table 1A.

Table 1A: Two Way Anova of Gross Message Length

Source	SS	df	MS	F	p
Total	630.23	33			
Sex of Speaker	100.47	1	100.47	7.76	.01
Magazine	23.06	1	23.06	1.76	NS
Interaction	113.74	1	113.74	8.68	.01
Error	392.96	30	13.09		

Post hoc t-tests yielded statistical significance ($p < .05$) between the following groups: Playboy male speakers had significantly longer messages ($X = 14.45$) than Playboy female speakers ($X = 11.00$) and LHJ female speakers ($X = 9.87$). LHJ male speakers had significantly longer messages ($X = 13.11$) than Playboy females ($X = 11.00$); but there was not significant difference between LHJ males and females. This explains the significant interaction F ratio.

⁹ A two x two analysis of variance of message length using sex of speaker and sex of receiver as the independent variables yielded the results summarized in Table 2A.

Table 2A: Two Way Anova of Gross Message Length

Source	SS	df	MS	F	p
Total	630.23	33			
Sex of Speaker	100.47	1	100.47	7.99	.01
Sex of Receiver	20.70	1	20.70	1.64	NS
Interaction	131.79	1	131.79	10.48	.01
Error	377.27	30	12.57		

Post hoc t-tests yielded statistical significance ($p < .05$) between the following groups: men speaking to men ($X = 14.57$) had significantly longer messages than women speaking to men ($X = 10.72$) or women speaking to women ($X = 9.00$). No other differences emerged.

10 Simple z tests for proportions ($p < .05$) provided the basis for these conclusions.

11 Simple z tests for proportions ($p < .05$) provided the basis for these conclusions.

12 The conclusions are based on two analyses of variance of the emotional ratings, one for each magazine. As summarized by the tables below, neither yielded any statistically significant differences among the six emotions employed in the ratings.

Table 3A: Analysis of Variance of Playboy Ratings

Source	SS	df	MS	F	p
Between	2.45	5	0.49	0.19	NS
Within	71.11	28	2.54		
Total	73.56	33			

Table 4A: Analysis of Variance of LHJ Ratings

Source	SS	df	MS	F	p
Between	6.34	5	1.27	1.63	NS
Within	21.90	28	0.78		
Total	28.24	33			

BUT HOW DOES A BILINGUAL FEEL?
REFLECTIONS ON LINGUISTIC ATTITUDES OF IMMIGRANT ACADEMICS

Lili Rabel-Heymann
University of Calgary

Bilingualism is a hot topic in Canada today, regardless of whether you refer to a native Quebecois who is equally fluent in French and English like our Prime Minister, or whether you mean an Anglophone Government official who has just struggled through a course in elementary French. I am not concerned with that kind of bilingual; neither am I concerned with a person who grew up in a speech community where two languages are used side by side and where the general population is completely at home in both languages, such as in some parts of Belgium, Finland, Switzerland, or Africa.

I am concerned with academically trained adults engaged in university teaching who immigrated to North America after the age of twenty, who settled in Anglophone Canada, and whose working language has become English, but who continue to make active use of their native language. I will not speak about the usual interferences due to language contact, such as phonetic substitutions, faux amis, or certain well-known transferences in grammatical structure. In other words, I will refrain from discussing those features of language interference that can be measured through experiments and tests.

Rather I will discuss the linguistic choices such immigrants make in certain given situations, some of their emotional reactions to their two languages, and I will point out certain pockets of resistance to complete linguistic acculturation that most adult immigrants seem to possess. Much of what I am going to say is based on my own thoughts and prejudices; in order to test the validity of my own attitudes I distributed questionnaires to 30 of my foreign-born colleagues at the University of Calgary. 21 questionnaires were returned, some filled out incompletely. This is understandable since I did not limit the distribution to people of my own ethnic background, not even to immigrants from Europe, but I asked colleagues from twelve different nationalities.¹

In the appendix I have tabulated about half of the answers. My colleagues and I do not agree in all instances because language choices are conditioned by several factors: the age at which the professor came to Canada; the native language of his spouse; the language used with his children, and the reasons for leaving his native country. Therefore some of my questions were not suitable for some of the respondents.

Before discussing my own observations I must say a few words about my own relationship with the English language. I first heard it spoken at the age of nine when an English family moved into our apartment house in Munich. I sometimes accompanied their three small children and their nanny to the "English Garden" and simply by listening to **their conversation** I fell in love with the sounds of English. Formal English study in school began later that year, was interrupted for seven years due to my family's move to Berlin and another school system, but was reinforced after graduation by a sojourn with an English family in Cambridge, was kept alive by reading English novels during the period when Germans could no longer travel abroad, it was revived, Americanized and intensified by my move to the United States and subsequent university education. In other words, English has been on my mind and in my heart for a long time; I still love it as much as ever and I will never give it up.

Unlike many German immigrants I have not stopped using my native language, I do not speak it with a fake American accent, and I do not begin my German sentences with "vell..." But English is my primary language. My criteria for establishing linguistic primacy are strictly subjective and differ from those Lambert (1972) used in his Québec tests: I talk to myself in English, I dream in English, I write notes and memos to myself in English, I prefer writing English letters, and when in great pain I find it easier to moan in English.

You may laugh at my admission that I talk to myself. I find it hard to live in a silent house and I enjoy the sounds of language. Perhaps, conditioned by the knowledge that the spoken word is a better reminder than a fleeting thought or a scribbled note, I have adopted the habit of conversing with myself, leading a Selbstgespräch. This habit may go back to my practice of studying the vocabulary and paradigms of foreign languages out loud. Russel Baker, the New York Times humorist, agrees with me, he recently wrote a column on "summer bachelors" saying that the worst part was the lack of conversation. Only six of my colleagues indicated that they did not engage in soliloquies.

Since dreams are hard to remember and since it is often impossible to decide whether the dream action involved language at all, I decided to write down some of the things I said to myself. Item 1: I am preparing dinner, talking to the pots and the food in them, I say "salad - mit einem Ei - why not?" Item 2: I am trying to kill a fly that's buzzing against a large window pane; I talk to the fly "where are you now? come here, sit down, sit still, da ha'm wir dich (I hit it)" and as I carry it out sticking to the fly swatter I say "degoutant" - in French. Item 3: I look at a list of houses for sale in our city and I hear myself exclaim "hundertneunundzwanzig tausend Dollar - for a three bedroom bungalow?" Item 4: I read about the losses the Calgary Convention Centre has suffered "four point seven million dollars -

die sind ja wohl verrueckt!" I was shocked to discover such undisciplined conversations with myself; but I know that this sort of alternating languages has never occurred in the company of others which seems to indicate that my internal stimulus is bi-rather than unilingual.

My own language mixture differs from the morphologically and lexically garbled language many half-educated German immigrants practice; I heard a German lady in Calgary say recently "ich hab's von den Vitnessen gerented" ('I rented it from the (Jehovah's) witnesses'), and another lady is reputed to have said "wenn ich einen guess venturn darf..." ('if I may venture a guess')

Speaking of myself, there is then a residue of German mental activity that acts up when I am alone; this residue may be related to certain cultural customs that an adult is unwilling or incapable of changing. In two activities I perform in German exclusively, not for linguistic but for emotional reasons: prayer and arithmetical calculations. I add, subtract, multiply, and divide in German reciting out loud the same fixed formulae that I was taught in grade school; they work faster than the bank teller's adding machine; 50% of my colleagues did the same. The question on prayer language was a touchy one; for that reason I had suggested to my respondents to omit their names. The majority say no prayers and those who do use overwhelmingly their native language. Most of the Calgary professors also stated that they preferred poetry in their native language, a preference which I do not share.

Another activity where immigrants retain native behavior is that of giving directions; Europeans prefer to say "turn left, right, or straight ahead" while North Americans say "turn south, north, east, or west." Three quarters of my colleagues, myself included, continue to use "left" and "right".

How then can we account for native language preference in praying, calculating, and poetry enjoyment? These activities were taught early in a child's life, probably by oral recitation with much repetition until they became automatic. Native language choice over an acquired second language - although the latter may have become "primary" - functions perhaps like certain cultural habits that are hard, if not impossible to change in adult life: food dislikes (I still don't eat pumpkin pie or peanut butter), a gentleman must walk on the left of his lady rather than on the outside, the gentleman speaks first and not the lady, or the handling of knife and fork when eating.

Another language use that involves emotion is the use of swear words; most of us agree that we know more of them in our native languages; the two who can swear better in English had served in the US Army! On the other hand, my colleagues, sixteen of whom are men, prefer to insult a person in English while I

simply lack the appropriate English vocabulary. Quite recently I was enraged at a man who dented my car by giving it a vicious kick; I wanted to shout "Sie unverschämter Luemmel, Sie, was faellt Ihnen denn ein?" but the English equivalent 'you insolent fellow' seemed so utterly unsuitable that I was left absolutely speechless.

All of my colleagues agreed with me that there are concepts for which English has no words: French sympathique, German Gemuetlichkeit, Italian ambiente, etc. Most of them, on the other hand, admitted that English words, such as 'drugstore' had to supply concepts, objects, or actions missing in their native languages. These findings are, of course, not surprising; the more languages a person knows, the more inadequate any one of them appears. It is only the monolingual who does not know what he is missing.

In my own English usage I miss for instance equivalents for German Ueberwindung, uebermuetig, fleissig, abgehaertet, sich anstellen. Even the new Muret-Sanders cannot satisfy me by listing 'effort' or 'will power' which literally translate Anstrengung and Willenskraft. It costs me Ueberwindung to step out of the open-air hot springs pool in Banff on a sub-zero, windy winter day - English lacks a noun to match 'overcome'. Uebermuetig is not the same as 'boisterous', 'rollicking', 'frolicsome', or 'full of fun' as Muret-Sanders suggests; perhaps a combination of all four. Uebermuetig includes good spirits, happiness, and mischievousness at the same time. In telling your seven-year old son "sei schoen fleissig in der Schule" you are simply admonishing him to do whatever is expected of him; but to say 'be diligent' or 'be industrious' to a small child is far too formal.

Another gap in English from the view point of some European speakers is the lack of diminutive suffixes; true, you show small children the kitty and the doggie, and little Bobbie hurt his footsie - but the '-ie' suffix is limited in distribution and does not fill the role of German -chen or -lein, not to mention the abundance of Italian and Spanish suffixes which change not only the size of an object but often its connotation. How much more picturesque does ein winziges Stuebchen sound than 'a tiny room' where you cannot add an -ie suffix.

Only rarely does the bilingual master slang in more than one language unless he resides in two countries for equal time periods. Although I still remember much of pre-war Berlin slang I am utterly ignorant of present-day German slang and am therefore deprived of a very colorful portion of native vocabulary.

Idioms, on the other hand, become a permanent part of a language, most of them endure and many are listed in dictionaries, yet they are very hard to acquire by the foreigner.

Hardly ever are idioms lexically equivalent in two languages, unless they are literal translations such as English 'that goes without saying' from French ça va sans dire; or perhaps German Recht haben from French avoir raison. I feel a certain frustration in not having equivalents in my two languages available at all times, and I am often tempted to make up idioms when the need arises; in the case of "ich koennte die Waende hochgehen" I am not sure whether English "I am going up the walls" is my invention or legitimate English usage.

Proverbs behave much like idioms, i.e. they can be learned; but they too are hardly ever literal equivalents in two languages. How is a German immigrant to know that his pun on ein Spatz in der Hand ist besser als eine Taube auf dem Dach, namely ein Dienstaedchen in der Hand ist besser als eine Komtesse auf dem Dach ('a servant girl in your hand is better than a countess on the roof') will fall flat because in English you must say 'a bird in the hand is worth two in the bush'?

And there is the field of puns and plays on words. It is the rare immigrant - such as many of our multilingual linguistic colleagues - who can be creative in two or more languages.

Probably the last sphere in which an immigrant learns to assimilate to a second culture is body language. Observation of my colleagues' kinesic behavior has so far not been possible so that I cannot make any statements about them. Since most of our body movements during the speech act are involuntary it is virtually impossible to be aware³ of one's own gestures. Only once has it been pointed out to me that I had unconsciously adopted an American gesture (for which I had no need whatsoever): in demonstrating how to reprimand a naughty child I was moving my right index finger sideways while the customary movement for Germans (in saying "du-du-du") is up and down.

I find it desirable to blend totally into a second, or even third language and culture when living in a new country and speaking a new language. This helps me to experience all aspects of life in the new culture with the eyes of the native. In most studies of second language learning this attitude is said to produce "anomie" or a conscious or unconscious abrogation of one's native culture. I beg to disagree with this interpretation. I think an immigrant can adopt and integrate into a new culture and yet retain respect and a certain affection for his native country and, with a little effort, proficiency in his native language. I therefore aim at nothing less than native performance, especially in pronunciation. But when I heard myself on tape for the first time I realized I had not quite succeeded, and that hurts. I dislike a German accent in English more than an English accent on German; but I dislike it even more when people who do not know any German at all pretend to speak German by croaking ach-ach and repeating "hossenfeffer"

(Hasenpfeffer - a rather uncommon word) over and over - that's what Merv Griffin did in one of his recent talk shows attempting to demonstrate the unpleasant sounds of German to a German movie actress.

Another pet dislike of mine is the American girl's name /grɛʃən/ from German Gretchen, a name suitable for little girls in pigtails or for Goethe's unfortunate heroine in Faust, but not for a middle-aged housewife. I once knew a girl named /ʃá:tsiy/ (from German Schatzi) literally 'little treasure', a term of endearment between young lovers, but most unsuitable for the tough nurse she wanted to become. I do not know whether native speakers of English feel similar resentment against German mispronunciations or misuse of English names.

Half of my colleagues stated that some English vocabulary had intruded into their native languages; I myself find it almost impossible to avoid the intrusion of such every-day words as gas station, campus, dry cleaner (for German Chemische Reinigungsanstalt), shopping center, and of course, o.k. Those who insist that their native language has remained pure are probably not quite honest with themselves.

Although most of the professors (18 out of 21) regarded a near-native command of English essential for the adult immigrant half of them admitted that they did not have such native command. 14 rated their pronunciation and nine their mastery of vocabulary as non-native; only one-third thought that they mastered English grammar as well as Anglophone Canadians - one thought he did better!

My own shortcomings include occasional confusion between cushion and pillow, pin and needle, prescription and recipe, i.e. vocabulary where two English words translate into one German word; a temptation to make up two-word verbs patterned on German cognate compound verbs such as 'pump up' for 'inflate'; and forgetting that English-speaking needles have eyes while German needles have ears (Nadeloehr).

Let me summarize: (1) Most foreign-born bilinguals are proud to have two languages and two cultures at their disposal; (2) Certain native language skills acquired orally in childhood, especially those with emotional involvement, will resist transfer into a new primary language; (3) I believe it is not possible for an adult immigrant to English-speaking North America to acquire a hundred percent competence in a second language on all levels; (4) It is much easier to switch political parties, national anthems, and passports than to replace Struwwelpeter by Mother Goose.

FOOT NOTES

1 Respondents to the questionnaire included one Italian, two Austrians, three Germans, three Swiss, four Czechs, two Ukranians, one Russian, one Frenchman, two Belgians, one Korean, two Indians, one Philippino. Two of these did not answer, but I do not know who they were. Six of the respondents were women, the rest men.

2 I remember a Latin American student who thought that 'phony' meant a ' little telephone'.

3 In 1949, after eight years in the United States, by Henry Lee Smith, Jr.

REFERENCE

Lambert, Wallace E., Language, Psychology, and Culture.
Stanford University Press. 1972

SELECTED QUESTIONS ON LANGUAGE ATTITUDES OF IMMIGRANT ACADEMICS

A. ACCULTURATION

	Yes	No	No Answer/Don't know
1. Do you consider it a drawback not to have spent your childhood and your high school years in Canada?	16	2	
2. Do you feel proud of having another culture at your disposal, i.e. is it an advantage to have a native culture and language in addition to those of Canada?	17	1	3
3. Do you at times feel a certain superiority over monolingual, monocultural Canadians?	11	10	
4. Do you think there is a definite relationship between a country's culture and its language?	16	3	2
5. Do you sometimes feel it is a drawback to be an immigrant in Canada rather than a native-born Canadian?	4	13	2
6. If a stranger in Calgary asks you for directions, do you direct him by saying "turn to your right, left" or by saying "turn east, west, south, north"?	8	12	1
	15	Either one	
	2	4	

B. LANGUAGE CHOICE

	Lg-1	Lg-2	Lg-1,2,3	No Answer/Don't know
7. If you are in the habit of talking to yourself when you are alone, in what language do you do it?	3	2	10	6
8. In what language do you say your prayers when alone?	7	1	3	10
9. Do you know in what language you dream?	1	2	12	14
10. In what language do you count, add, subtract, calculate, and figure your income tax?	10	3	8	6
11. In what language do you prefer to read or hear poetry?	14		3	4
12. Which language do you now consider your "primary" language, Lg-1 (native) or Lg-2 (English)?	10	6	1	4
13. If you are asked for your home address in Lg-1, in what language do you tell your house number?	6	13		
14. In what language do you know more swear words?	15	2	4	
15. When you are very angry at somebody, in what language do you want to insult him?	5	7	6	3

	Yes	No	No Answer Don't know
<u>C. LANGUAGE APPRAISAL</u>			
16. Do you feel a certain pride in being master of two or more languages?	18	3	
17. Do you think it is essential for an adult immigrant to Canada to acquire near-native proficiency in English?	18	3	
18. Do you feel you are in <u>complete</u> command of Lg-1 (native) and Lg-2 (English), at all times?	10	11	
19. Do you think your oral command of English is as good as that of a native Canadian? a. in pronunciation	6	14	
b. in grammar	14	7	
c. in vocabulary	12	9	
<u>D. LANGUAGE INTERFERENCE</u>			
20. Are there concepts of every-day living in Lg-1 for which English seems to have no equivalents?	20	1	
21. In a Lg-1 conversation with a speaker of your own native language, do you sometimes use English words like <u>frustrate</u> , <u>adjust</u> , <u>involve</u> because you cannot find Lg-1 equivalents?	14	7	
22. Do you often use English words in the middle of a Lg-1 sentence such as <u>car</u> , <u>shopping centre</u> , <u>movie</u> , <u>mailman</u> although you know their Lg-1 translations?	11	10	
23. Do you find that your daily use of English has interfered with your command of Lg-1 in a. vocabulary	11	)	) No Interference: 8))
b. word order	1	)	
c. pronunciation	2	)	
<u>E. PRESENT RELATIONSHIP TO SPEAKER'S NATIVE CULTURE</u>			
24. Do you have a feeling of satisfaction when you hear or read that your native country has performed a service beneficial to mankind?	19	2	
25. Do you have a sense of pride when a prominent person from your native country is singled out for praise or admiration here in Canada?	18	1	2

ELICITATION OF CULTURAL STEREOTYPES
THROUGH THE PRESENTATION OF VOICES
IN TWO LANGUAGES

Bernard Saint-Jacques
University of British Columbia

In 1967, Lambert¹ had pointed out the usefulness of the matched guise technique to reveal cultural stereotypes which, in some cases, are not even perceived by the subjects themselves. This is particularly true when languages and cultures are in contact. However, two basic difficulties are related to the use of the matched guise technique. First, perfect guises in two languages are very difficult to find. This is particularly true of the languages of the experiments presented in this paper: Japanese/English and Chinese/English. The second difficulty stems from the fact that subjects easily identify the voice of a "perfect guise" reading a passage in two languages, even when the occurrences of the two readings are interrupted by other voices. To avoid these difficulties, only native speakers were selected. They were asked to read a text in their mother tongue in successive turns with native speakers of the other language, reading a translation of the same text.

Experiment I.

The First Experiment was conducted in Japan, at the International Christian University, during the Language Sciences Summer Institute (July-August, 1976). The subjects were 100 native Japanese students attending the Summer Institute, 50 males and 50 females. Two age categories were retained: 1) 0 to 25 = 59 subjects, 2) 26 to 45 = 61 subjects. 6 male voices were recorded: 3 native American speakers and 3 native Japanese speakers. Efforts were made to select the 6 speakers from similar age and educational backgrounds. All spoke a standard form of English or Japanese. The passage to be read was an excerpt from the ICU calendar describing the setting and the facilities of the University (See Appendix I). This text exists in English and Japanese. The speakers were told about the purpose of the experiment and were given some time to read the text. Then, they were recorded.

¹W. Lambert, "A Social Psychology of Bilingualism", Journal of Social Issues, 23, 1967.

On the tape, preceding the voices of the speakers, a Japanese (female) gave some brief explanations in Japanese about the experiment and the way it is conducted: "You will be participating in a study designed to investigate the voice and its influence on our perception of others. For example, we have all experienced the tendency to form judgments about people sight unseen as is the case over the telephone or on the radio. These judgments often concern the speakers' looks, sociability, kindness, etc... You will hear six Speakers numbered from 1 to 6. Each Speaker will read the same text in either English or Japanese. Speaker No. 1 will read the text in English, Speaker 2 will read the same text in Japanese, and so on in the same alternate fashion. After each reading, there will be a brief pause giving you time to rate the speaker according to the characteristics listed on the handout. Put an X in the box indicating the degree to which the particular characteristic applies to the Speaker in question." There were 8 characteristics 1) good looks, 2) height, 3) kindness, 4) sense of humour, 5) sociability, 6) frankness, 7) diligence, 8) logic, and four degrees: not at all, a little, average, and very. Before the test, subjects were given a handout with one page for every speaker, where characteristics were clearly listed with boxes indicating the degrees. Each reading took approximately 60 seconds. A pause of 30 seconds was left between each speaker. The 8 characteristics were selected after many discussions with Japanese. It was their consensus that these characteristics were related to basic stereotypes in the perception of Americans and Japanese by Japanese. It should be noted that in the brief instructions to the subjects, cultural stereotypes, subjective judgements about races, etc., were not mentioned. On purpose, the attention of subjects was shifted to the "investigation of the voice and its influence on our perception of others".

Results of Experiment I.

On 6 characteristics, American voices were evaluated more favorably than Japanese voices: good looks, height, sense of humour, sociability, frankness and logic. Three of these six characteristics showed a very significant difference: height, good looks and logic. The first one was expected. In general, Americans are taller. Is the second one, "good looks", related to "height"? It is interesting to notice that on these two characteristics, although male and female subjects evaluated American voices more favorably than Japanese voices, female subjects showed a far greater difference, particularly on "good looks". The third characteristic "logic" is the English translation of the Japanese "ronrisei". It implies the capacity of making decisions on pure logical grounds, without the interference of sentimental reasons. All subjects were unanimous (males and females) in attributing this quality to a higher degree to American voices. This particular result might seem strange to

someone not familiar with Japanese culture. It is however indicative of a basic cultural difference between Japanese and Americans which cannot be developed within the scope of this paper.

On two characteristics, diligence and kindness, Japanese voices were evaluated more favorably than American voices. These results are also related to important cultural differences and offer valuable insights in the self-perception of Japanese as well as their perception of Americans. Concerning "diligence", the rating of male subjects is extremely high compared to the rating of female subjects. In other words, male subjects indicated that Japanese males are extremely diligent when compared to American males. Females subjects noted only a slight difference. This demarcation between the results of male and female subjects is also evident on "kindness". The overall score on this characteristic favored Japanese voices, but it is caused by the high rating of male subjects. On this characteristic, female subjects slightly favored American voices.

Experiment II.

The second Experiment was conducted at the University of British Columbia in March 1977. The subjects were all university undergraduate students. They were composed of 80 English Canadian subjects (60 females and 20 males) and 40 Chinese Canadian subjects (20 females and 20 males). Four voices were recorded, 2 native speakers of Canadian English and 2 native speakers of Chinese (Mandarin). The text used was taken from Noam Chomsky's Syntactic Structure (1957) and the same passage in the Chinese translation of the book (See Appendix II). Eight characteristics were chosen: 1) diligent-hard working, 2) family-oriented, 3) expressiveness-openness, 4) disciplined, 5) good-looks, 6) community-oriented, 7) politeness, 8) thrifty-economical. The technique and the presentation of the test were the same as in Experiment I.

The predictions, corresponding to stereotypes in the Greater Vancouver area community, were as follows:

- a) Chinese voices would be rated higher on the following characteristics:
 - diligent-hard working, family-oriented, disciplined, community-oriented, politeness, thrifty-economical.
- b) Chinese voices would be rated lower on the following characteristics:
 - expressiveness-openness, good-looks.

On the first characteristic "diligent-hard working", the prediction did not materialize and no significant difference was found. On the "family-oriented" characteristic, Chinese speakers

rated higher as expected. The third characteristic "disciplined" did not reveal any significant difference. "Community oriented" and "politeness", did not present any overall significant difference, however, on the first one, "community-oriented", a greater percentage of English males rated Chinese voices higher; on the second one, "politeness," a greater percentage of English females rated Chinese speakers as very polite. No difference was found on the characteristic "thrifty-economical".

The overall scores on "expressiveness-openness" and "good-looks" did not support the prediction that Chinese voices would be rated lower on these two characteristics, but English voices were rated higher on good-looks by English female subjects.

As a whole, the only characteristic strongly supported by the results of Experiment II is "Family-oriented". Chinese voices are rated more highly on this characteristic than English voices by English male and female subjects as well as by Chinese female and male subjects. These results could indicate that many of the traditional cultural stereotypes concerning Chinese Canadians and English Canadians in the Greater Vancouver area are not alive any more in the minds of young people. It could reflect the general tendency of Canadians in Vancouver, whatever their ethnic origin, to consider themselves as Canadians with less emphasis on the hyphenated part: Chinese, Japanese, German, English, etc...¹

CONCLUSION

There is no doubt that the elicitation of cultural stereotypes and subjective judgments could be of great importance for the learning of second languages, the understanding of another linguistic group and, in general, for sociolinguistic analysis. From the results of these tests, one can often find stereotypes which are not explicit in the community, or are not perceived by the speakers themselves. Moreover, these tests might throw new light on the diachronic changes affecting cultural stereotypes. Cultures are not static, cultural stereotypes are not either.

The results of these two experiments could justify a more thorough analysis than the one allowed by the time limitations of this paper. For instance, in Experiment I, the high rating of American voices on the characteristic "logic" by all Japanese subjects is quite significant. Also the sexual differences of Japanese subjects in the rating of American and Japanese voices on "diligence" and "kindness" could motivate interesting research.

¹ See B. Saint-Jacques, Aspects sociolinguistiques du bilinguisme au Canada, International Center for Research on Bilingualism, Quebec, 1976, Chap. VIII, "Les mécanismes d'assimilation au groupe anglophone".

Concerning the results of Experiment II, the lack of support for many of the traditional cultural stereotypes attributed to the Chinese community could indicate either a change in patterns of living of the Chinese themselves, or a change in the perception of these patterns by the English community, or both.

In both experiments, one voice obtained an overall score significantly higher than other voices, irrespectively of languages (In Experiment I, Speaker 4 (Japanese) - out of six Speakers; in Experiment II, Speaker 4 (Chinese) - out of 4 Speakers). The reasons for these differences could not be accounted for and could be the object of further research.

ACKNOWLEDGMENT

The author would like to express his gratitude to the various students at ICU and UBC who have helped him with the administration and the computation of the tests, particularly, Ayako Shinomiya and Kasuhiko Sato of ICU, Melodie MacFarlane and Odette Jobidon of UBC.

Appendix I

The International Christian University is located on a spacious wooded campus of 1,005,000 square meters in suburban Mitaka, about one hour from central Tokyo. The upper level is the site of the academic buildings and dormitories; faculty residences line the ridge; the lower level lands have been made into an 18-hole golf course for the past twelve years as an interim project to utilize the area of the University's property. However, the ICU Golf Course will formally close this year, with only the practice driving range to continue in operation. The University has transferred a section of the Golf Course to the Tokyo Metropolitan Government for use as a nature preserve under the name Nogawa Park. From the ridge one looks over the plain to the Chichibu range of mountains in the West; majestic Mount Fuji beyond can be seen on a clear day.

Appendix II

One requirement that a grammar must certainly meet is that it be finite. Hence the grammar cannot simply be a list of all morpheme (or word) sequences, since there are infinitely many of these. A familiar communication theoretic model for language suggests a way out of this difficulty. Suppose that we have a machine that can be in any one of a finite number of different internal states, and suppose that this machine switches from one state to another by producing a certain symbol (let us say, an English word). One of these states is an initial state; another is a final state. Suppose that the machine begins in the initial state, runs through a sequence of states (producing a word with each transition), and ends in the final state. Then we call the sequence of words that has been produced a "sentence". Each such machine thus defines a certain language; namely, the set of sentences that can be produced in this way. Any language that can be produced by a machine of this sort we call a finite state language; and we can call the machine itself a finite state grammar.

CHILDREN HAVE BODIES

Walburga von Raffler-Engel

Vanderbilt University

Of all scholars working with language the psycholinguists have formulated the least workable theories, theories that are not consonant with reality. Saussure posited the existence of a system of rules underlying the observable phenomena, and the American linguists working in psycholinguistics before this interdisciplinary field became the exclusive domain of the Chomskyan school of psycholinguistics believed that any structure must be derived from factual data. After the failure of the transformational model, it became clear again that theories of language acquisition, speech programming, and speech perception must take into account the nature of man and the essential characteristics of language. Given that there are personality differences among infants (von Raffler-Engel 1968, Nelson 1973) and that in later life individual differences are even more pronounced, it becomes impossible to account for every speaker's linguistic competence with one and the same model (von Raffler-Engel 1969). Even within the individual, changes occur over time. In bilinguals, in particular, one can observe "changes over time in their various interlingual relationships." (Paradis 1977).

All languages have changed over time and there is no language that does not affect internal variation (Anttila 1972). Language itself is open-ended and, to quote Hall (1977), not wholly neat, logical, coherent. Bolinger (1975) pointed out that it is its "something-like principle" which allows language to function. Language could not have a well-defined system and, as shown by Hockett (1969), indeed does not have one. The "messiness" of language, to use Werner Winter's (1965) expression, has to be incorporated into any theory of psycholinguistics. Most importantly, if contradiction obtains between the evidence and the linguistic analysis, the theory and not the data must be adjusted. By data I mean all the data. The concept of "marginal data" proposed by the transformational school of language acquisition (Menyuk 1969) must be rejected. The data must not only be complete but also presented within their context and situation. In the context of a preceding conversation about the upcoming visit of one's aunt, the celebrated sentence that "visiting relatives can be a nuisance" is as unambiguous as when it is uttered in the situation of that same overbearing aunt just having left the room. Language cannot be adequately analysed when it is isolated from the interactional process in which it occurs. No first language, be it the spoken language of the normal hearing child or the sign

language of the deaf, can ever be acquired by a child without the presence of another human being. The mother-child relationship is the basis of language acquisition (von Raffler-Engel 1964). As Nakazima (1976) phrased it, through the affective relationship between mother and child the emotional symbolism is formed. Eventually, the child starts to differentiate the signifier from the signified and the symbolic function in the sensory-motor intelligence is formed.

The emotional, bodily and verbal immediate interactant--usually the mother--is part of the child's environment. To complete the presentation of the data one has to describe also the environment-at-large, other human beings, food, animals, artifacts, and whatever stimulants the child has to think and talk about. The items are as visible in the room as the words are audible. The visible context and the situation (time to go out or the like) are part of the speech event. If they are not mentioned they need not be underlying the linguistic structure. These shared presuppositions are not necessarily verbalized even in the adult. Language data are also incompletely presented when they are severed from the concomitant bodily behavior of the conversation partners. In the past linguists have analyzed language as if the spoken word were the only means for conveying a message. They ignored the fact that human communication is a combination of sounds and gestures.

Why did linguists take so long before they incorporated gestures into the analysis of language? Even those anthropological linguists who actually looked at their informants did not analyse body movements in their own right. Most of the time, they jotted them down on their note pads to serve as a prop when they would go down to their real business of analysing the verbal transcripts. In addition, the bulk of their transcripts came from monologs. Linguists seemed to ignore the fact that most of natural language occurs in the dialog, within a conversational setting.

Even more strangely, the psycholinguists working with infants by and large ignored the obvious: eye contact and touch between mother and child, not to mention the infants' pointing at objects. Children, like adults were treated as if they were bodiless except for the mouth.

What all these linguists did was to transcribe what they heard and then work on their transcript as if it were the real thing. What they did not realize is that spoken and written language are different media of communication. When we read in a piece of literature that the professor asked the student what he had come for, all the while keeping an eye on his watch, we realize that this professor did not encourage the student to speak. When we look at the linguist's transcript of such a speech act, we find no mention of the professor's gazing at his

watch. An incomplete transcript makes a fully accurate interpretation impossible. The non-verbal component of human communication is as vital to the message as the verbal component.

Considering the essential unity of verbal and non-verbal behavior in communication, one wonders how these two components have ever been so compartmentalized as if each had an independent function rather than constituting merely one of the two aspects of a combined function. When a small child points to an object while uttering his carrier-sound--and eventually the correct noun--with rising intonation, he indicates his desire for that object. When the pointing gesture and the paralinguistic aspect of intonation are included in the analysis of the child's communicative behavior, one can discern three overt features to convey the meaning. Theories postulating deletions from underlying syntactic structures appear unnecessarily powerful. As children build up their verbal grammar, the pointing gesture from obligatory becomes optional.

The area of non-verbal behavior is still new and the terminology is still in flux. Various ways for subdividing the area of non-verbal behavior and diverse strategies for their analysis have been proposed. A survey of these approaches has been prepared by Randall Harrison (1972). For the specialized field of language acquisition I would like to propose the following tri-partite division. Body language for bodily movements in general as they express the mood of the individual, nervously walking back and forth, fidgeting with his hands or crossing his legs for a more comfortable sitting position. Kinesics proper concerns message-related body movement which can substitute for a verbal expression, or accompany a verbalization, reinforcing it. A German speaker can simply touch his forehead with his index finger or while doing so may say that such and such a person is a bit nutty. Mediterranean people have a rich repertoire of kinesic gestures; Anglo-Saxons are supposed to "keep their hands still;" and in one African language, numbers are always indicated by the fingers. (Alexandre 1972)

The third category is probably the most important one. It comprises those movements of the body that are obligatory within social interaction. These are movements which children may be explicitly taught while kinesic motions are generally acquired by osmosis like verbal language. I would propose to call them social movements. Greeting formulas enter into that category. In Europe upper class youngsters learn how to kiss a lady's hand without their lips actually coming into physical skin contact. Black Americans have developed the highly stylized art of the "soul handshake." The hand may touch any part of the forearm and even the elbow, giving a warm sensation of companionship. Among Orthodox Jews, members of the same sex may hug each other, but men do not shake hands with women. The Japanese, who find the custom of hand shaking distasteful and unclean, greet each other

with a deep bow. Persons of equal status get up at the same time--not an easy feat to accomplish because the two parties must ogle each other unobtrusively in order to synchronize their movements.

In non-verbal behavior, like in all forms of human behavior, there are elements that are innate and common to all men and there are specific forms which are culturally conditioned. Children are socialized into their culture from the moment of birth. In North America newborn babies are separated from their mothers and bottle fed by a professional nurse at regular intervals. In other cultures babies enjoy a playful body touch and rhythmic rocking by their mothers, who sing to them.

As children grow up they adjust to the kinesic behavior of their community. Eventually they establish a pattern of expectancy for gestural behavior similar to the expectancy which makes one's mother tongue understandable and other languages foreign. In the Vanderbilt Linguistics Program we tested this expectancy by showing a videotape of a conversational interaction involving a certain amount of gesticulation. Persons from low-kinesic cultures hardly noticed any details of the ongoing gesticulations while persons from high-kinesic cultures, such as South Americans, recalled most of the movements. Bicultural individuals have more than one kinesic code just as bilinguals possess automatic mastery over more than one language.

The development of non-verbal behavior exhibits the delicate interplay of the child's maturational curve and his gradual acquisition of the social rules which govern body movement within his culture. Japanese children gesticulate very freely when they are young, but they suppress many of these gestures as they grow up. Italian children need to learn less self-control in their body movements, but they do have to acquire a great many specific forms of semiotic gestures.

Kinesically, small children of all cultures tend to make use of self-centered (distractive, nonconforming) attention-getting devices. Older children appear to favor the other-centered (cooperative, conforming) attention getting device, which is the form generally used by adults. The small child is also unaware of the requirements of kinesic synchrony, an automatic conformity of posture and hand and foot position within a group of interactants.

After a certain age, children will automatically fall into kinesic synchrony. Nevertheless, in cultures where women are not supposed to sit in ways which are considered masculine, girls will not assume these seating positions. Within the constraints of biological maturation, body movements are as much learned as verbal language (von Raffler-Engel, 1976).

My then student, Mrs. Claire Lerman, and I prepared the following graph for Developmental Kinesics (von Raffler-Engel 1974).

See "The Presence of Communicative Skills in Kinesics at a Given Age" Chart at the end of article now.

The cultural constraints are stronger than the instinctive desire for kinesic synchrony.

Whether there is a critical age or an optimal age for language acquisition is a much debated question (von Raffler-Engel 1975). In regard to that same question concerning the acquisition of kinesics, the current state of our knowledge is virtually nil. Given the inseparable relationship between words and gestures and the synchronous acquisition of the forms and the use of the means of communication, it is likely that the answer to the problem of critical age for language and for gestures will be the same.

When an adult interacts with a child he tries to accommodate his non-verbal behavior to the age of the child. The adult will sit close to a small child, touch the child when he feels that the little one's attention is waning, and make use of extended movements of the hand and the arm to illustrate what he says (Hopson-Hasham 1977).

Beyond the gestural behavior of people in conversations with each other, one can also research how people perceive and eventually reproduce the kinesic behavior of a third party. In my research I observed that people will straighten their posture when talking about somebody who usually sits in a very formal manner. On the analogy of metalanguage I decided to call this behavior metakinesics.

The relationship between the original kinesic movement of the model and the metakinesic movement of the reproducer is not one of absolute identity, but rather one of sameness of proportions. The speaker who reproduces a kinesic model does so within the limits of the space available between himself and his conversation partner. His reproduction may be faster or slower and larger or smaller than the model, but it tends to maintain the same proportional relationship which obtained in the original between the duration, range, and direction of the movement (von Raffler-Engel and Weinstein 1977).

See "The Process of Metakinesic Programming" Chart at the end of this article now.

The metakinesic chart which emerged from my research with adults in the process of describing the body motions of children may be helpful as a basis for the description of the body motions of children in the process of being inculturated into the conversational habits of their elders. The exigencies of space do not permit me to outline here the methodology and research strategies for Developmental Kinesics. The point I have tried to make in this paper is that the acquisition of verbal language should be studied jointly with the acquisition of non-verbal communication. Tape recording does not provide sufficient evidence. The use of video equipment or film is essential.

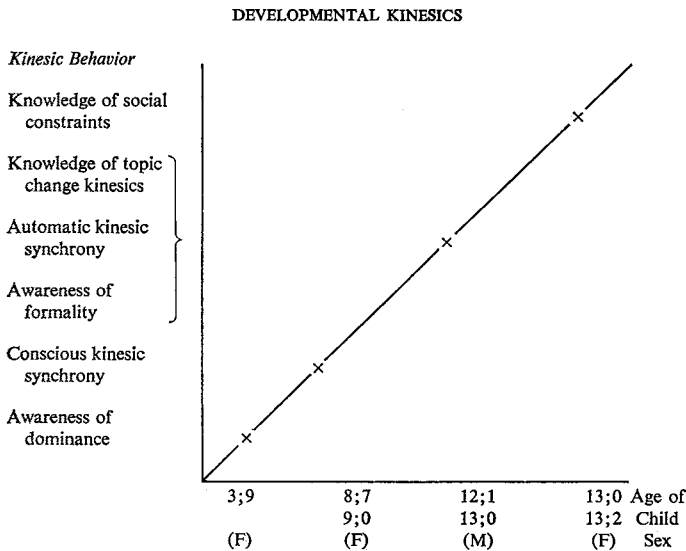


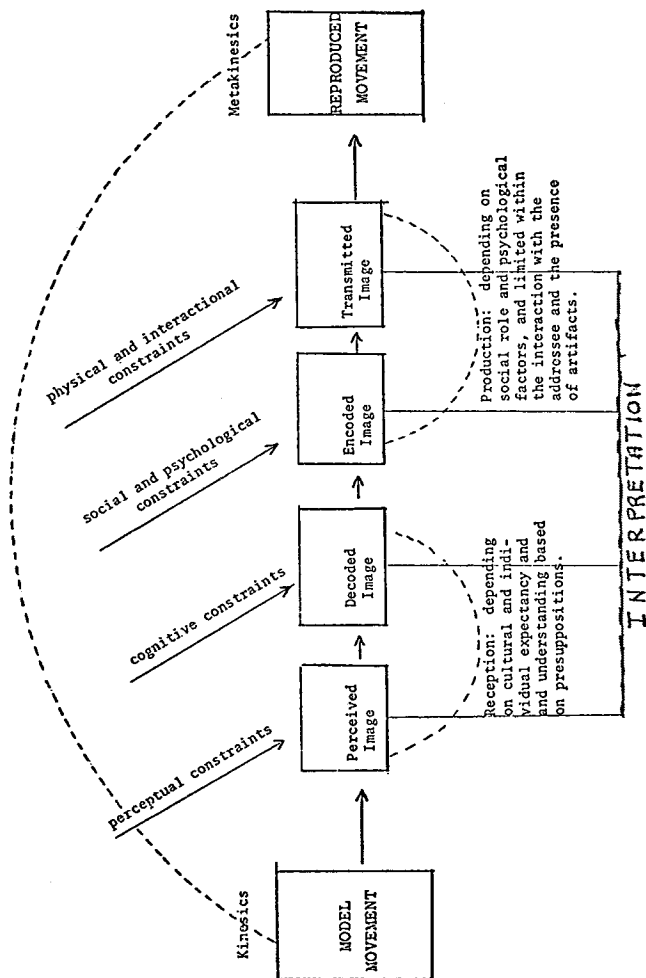
CHART 2. The presence of communicative skills in kinesics at a given age*

* This graph is presented not to indicate precise relationships but to clarify relationships of variables found in a limited sample. It indicates the presence of skills at a given age, not at the time of first appearance.

Psychokinesic chart

Walburga von Reffler-Engel (Vanderbilt University)

The Process of Metakinesis Programming



References Cited

- Anttila, Raimo 1972. An introduction to historical and comparative linguistics. New York: MacMillan.
- Alexandre, Pierre 1972. An introduction to languages and language in Africa. Translated by F. A. Leary. London & Ibadan, Nairobi: Heinemann. (Original title: *Langue et langage en Afrique noire*. 1968.)
- Bolinger, Dwight 1975. Aspects of Language, 2^d ed. New York: Harcourt, Brace & Jovanovich.
- Hall, Robert A., Jr. 1977. "Some critiques of chomskyan theory" in *Neuphilologische Mitteilungen* 1/LXXVII, pp.86-95.
- Harrison, Randall P. 1972. "Nonverbal behavior an approach to human communication" in Approaches to human communication ed. by R. W. Budd & B. D. Ruben. New York: Spartan Books, pp. 253-268.
- Hockett, Charles F. 1968. The state of the art. The Hague: Mouton (Janua Linguorum, Series minor no. 73).
- Hopson-Hasham 1977. "The adjustment of the speaker to the hearer" in Aspects of non-verbal communication ed. by W. von Raffler-Engel & B. Hoffer. San Antonio, Texas: Trinity University Press.
- Nakazima, Sei 1976. "A comparative study of the speech development of Japanese & American children (part 7)" in Studia Phonologica X pp. 16-22.
- Nelson, Katherine 1973. "Structure and strategy in learning to talk" Monographs of the Society for Research in Child Development 38/1-2, Chicago: University of Chicago Press.
- Paradis, Michel 1977. "The stratification of bilingualism" in The third LACUS Forum 1976, Columbia, S.C.: The Hornbeam Press.
- Menyuk, Paula 1969. Sentences children use. Cambridge, Mass.: the M.I.T. Press (Research Monograph No. 52).
- von Raffler-Engel, Walburga 1964. Il prelinguaggio infantile, Brescia, Italy: Paideia (Studi grammaticali e linguistici).

- _____. 1968. "Suprasentential and substitution tests in first language acquisition" in Bollettino di Psicologia Applicata 88-89-90, pp. 33-41.
- _____. 1969. "Competence--a term in search of a concept," Mélanges Buyssens, ed. by Y. Lebrun, Brussels, Belgium: University of Brussels Press, pp. 271-285.
- _____. 1970. The LAD, our underlying unconscious, and more on 'felt sets'," in The Language Sciences, December, pp. 15-18.
- _____. 1974. Children's acquisition of kinesics. New York: Campus Film Distributors.
- _____. "The concept of critical age in language acquisition; a position paper" (Paper read at the VIIIth Annual Meeting of the Societas Linguistica Europea, University of Jyväskylä, Finland, 1974) in Folia Linguistica IX-1-4, pp. 17-27.
- _____. "Developmental kinesics: cultural differences," (Paper presented at the 25th Anniversary Meeting, International Communication Association, Chicago, Ill., 1975) in Child Language 1975, special triple issue of Word (XXVII, 1-3), ed. by W. von Raffler-Engel, 1976, pp. 195-204. New York: International Linguistic Association.
- _____. and Steven Weinstein 1977. "Metakinesic behavior in the description of non-verbal behavior." Paper to be presented at the XIIth International Congress of Linguists, Vienna, Austria: University of Vienna.

A FUNCTIONAL EMPIRICIST APPROACH TO THE ANALYSIS OF FACE-TO-FACE INTERACTION(1)

Madeleine Mathiot and Eleanor Dougherty

State University of New York at Buffalo

I Introduction

Scope of Paper

Our concern is with communicative behavior as it occurs when people engage in face-to-face interaction. Communicative behavior is viewed as a type of cultural behavior characterized by the fact that some interaction - be it immediate, delayed, reflexive, mediated, or extended - takes place between two or more people.(2)

We propose to investigate face-to-face interaction in its natural setting, i.e. both as it naturally occurs, rather than under laboratory conditions, and as it is culturally delimited rather than on the basis of illustrative examples. These conditions are realized in a multitude of culturally specified circumstances such as a meal, a meeting, a visit, a party, a ceremony, etc. The latter are designated as occasions of face-to-face interaction.

The aim of this paper is to consider the basic distinctions we propose as pertaining to the theoretical and methodological frame of reference for the analysis of communicative behavior in occasions of face-to-face interaction. Before we discuss these distinctions, let us first present the principles that guided our search.

Functional Empiricism

The general approach being followed is that of functional empiricism (see Paul L. Garvin's Inaugural Lecture). The name chosen for this approach reflects its two crucial characteristics. The empiricist aspect of the epistemology requires that all assumptions about the nature of the object of cognition and about the nature of the method used to deal with the object be derived from the observation of behavior and be susceptible of being referred back to the observation. Such a requirement implies a particular interdependence of theory and method. The functional aspect requires that a given phenomenon be studied in terms of its functions: how it functions as a whole and how its elements function with respect to each other. Primary importance, therefore, is given to function over structure and to meaning over form.

The relationship between theory and method is one of mutual and progressive reinforcement. The theoretical apparatus at the outset of research is little elaborated being comprised chiefly of the fundamental beliefs regarding the object of cognition. The methods devised for the investigation derive from this theoretical understanding of the object. The goal of the methodological appa-

ratus is not primarily to ensure the scientific respectability of the theoretical apparatus by testing the validity of the concepts pertaining to it, but rather to substantiate and refine these concepts. Theoretical detail emerges as a result of successive descriptive studies. Progressive increase in the specificity of the theoretical apparatus is directly connected with the corresponding development of the methodological apparatus.(3)

The primacy afforded function over structure and meaning over form is based on the conviction that cultural behavior is experientially organized in terms of function, i.e. categories of meaning, and it is on this basis knowable. It is to this culturally apprehended world that the individual relates. Meanings are here held to be the phenomena in terms of which interactive behavior is organized. Methodologically this requires that meanings, because they are not directly observable as are the physical signals necessary for their conveyance, must be inferred from additional kinds of data. These data, of necessity, consist of information collected from members of a culture familiar with the behavioral event under investigation since it is they who are privy to the meanings.

One of the fundamental assumptions of functional empiricism is that cultural behavior is "symbolic" behavior(4), i.e. that it carries information. As such, cultural behavior can be said to have both form and meaning. By form is meant substantive manifestation, i.e. concrete and consequently directly observable. By meaning is meant attributed content.(5)

The patterning of cultural behavior as specified above is believed to be rendered most tractable to systematic description from the perspective of two analytic notions: structure and function. A structure is viewed as being constituted of units that have both form and meaning and of the abstract relations obtaining between these units. Function is viewed as the part played by a given type of behavior in the life of the people who engage in this behavior.(6) In other words, functions are construed as organizing principles for the way in which cultural behavior is experienced by the members of the culture. As such, functions reveal a culture's world view.

Let us now examine briefly how the four key notions mentioned above, namely form, meaning, structure, and function, relate to one another.

The notion of structure is related to that of form in the sense that a structure has a substantive manifestation. Note, however, that no one-to-one correspondence is postulated between a particular substance and a structure. A given substance may be construed as the substantive manifestation of several structures. And conversely, a given structure may be manifested in different substances.

The notion of function is related to that of meaning as follows: function is attributed to a type of behavior as a class, whereas meaning is attributed to individual instances of a given type of behavior.

The notion of structure and function are related to one another

in a complex way. A function can either be the defining property of a structure or the attribute of a structure. As defining property, the notion of function is primary over that of structure. As attribute, the notion of function is secondary to that of structure. Functions on the basis of which structures are ascertained are called dominant functions. Functions that are attributes of structures are called subordinate functions. An instance of a dominant function is the referential function of the linguistic structure. The linguistic structure ascertained on the basis of the referential function has subordinate functions. Two types of subordinate functions postulated for the linguistic structure by Paul L. Garvin and the principal author are the transactional function and the personal function. An example of a transactional function is the group identification function of a standard language. An example of a personal function is tension reduction.

The notion of meaning is related to that of form as follows: form and meaning are viewed as two aspects of the units of the structures postulated as underlying the cultural behavior under analysis. This is the Saussurean position. The theoretical implication of this position is that no semantic units should be postulated as separate from substantive units. Only units that have both form and meaning should be postulated. Form and meaning are thus related through the notion of structure and consequently also through that of function. The functions, dominant or subordinate, associated with the postulated structures are uncovered by considering what types of meaning are conveyed through the behavior under analysis. Substantive differences in this behavior are regarded as relevant only if they are associated with differences in meaning. Thus the difference between saying, writing, or gesturing, "Come here!", is relevant only if a special meaning can be attributed to the use of the vocal substance as opposed to the graphic substance or the kinesic substance. The foregoing discussion of the relation between form and meaning, in which forms are viewed as the varying means whereby by meanings are realized, suggest that the notion of meaning is primary over that of form.

The conceptualization of the relation between form, meaning, structure, and function emerges from the conviction that cultural behavior is experientially organized in terms of categories of meaning and that, therefore, the analysis of face-to-face interaction should be based upon culturally relevant criteria. We hold that in order to be relevant an analysis should have psychological reality. By this we mean that it should reflect the way in which the members of the culture conceive of the cultural behavior under analysis. This view is radically different from the commonly held opinion that the relevance of a pattern is warranted simply by its having been uncovered by the analyst.

II

APPLICATION OF FUNCTIONAL EMPIRICISM TO THE ANALYSIS OF FACE-TO-FACE INTERACTION

The principles described in the previous section guided the search for the basic distinctions being sought for the analysis of

face-to-face interaction in two crucial ways: (1) in specifying the nature of these distinctions and consequently the task at hand; and (2) in specifying what data were more likely to provide the information pertinent to the task at hand.

Task at Hand

In light of the goal stated initially and the general approach being followed, the task at hand can be specified as one of ascertaining how many structures should be postulated as manifested through communicative behavior in occasions of face-to-face interaction. This formulation of the task implies that dominant, rather than subordinate, functions of communicative behavior are being sought since it is in terms of dominant functions that structures are ascertained. In view of the fact that an occasion of face-to-face interaction is a cultural unit, a further distinction suggests itself. This distinction is between the structure(s) postulated as underlying the occasion as a whole and the structures some pieces of which are utilized by the participants in the occasion as a means of communicating information. Thus in most occasions some pieces of the linguistic structure are used by the participants as a means of communicating information. These aspects of the linguistic structure cannot be regarded as the structure(s) underlying the occasion as a whole. The structures underlying the occasion as a whole are called macrostructures. The structures pieces of which are utilized by the participants in the occasion as a means of communicating information are called microstructures. In line with this distinction, two types of dominant functions are distinguished: dominant macrofunctions and dominant microfunctions. Dominant macrofunctions are those in terms of which macrostructures are ascertained. Dominant microfunctions are those in terms of which microstructures are ascertained.

The macro versus micro distinction has theoretical as well as operational implications. Theoretically this distinction is important because it implies two types of cultural objects: those which, like language, are only microstructures and those which, like occasions of face-to-face interaction, novels, plays, etc., are macrostructures and utilize microstructures. The former are sign systems. The latter are not. Note that this conclusion is not in keeping with the common assumption of semioticians that all cultural objects are sign systems. Operationally the macro versus micro distinction is important because it implies the sequence in which the analysis should proceed. The uncovering of the macrofunctions is logically prior to that of the microfunctions for the following reason: the microfunctions of communicative behavior are revealed by the way in which this behavior serves to convey the information specified by the macrofunctions. When these distinctions are taken into account, the initial task in the building of a frame of reference for the analysis of face-to-face interaction can be formulated in the following manner. It consists in ascertaining, as a first step, how many macrostructures, then how many microstructures should be postulated as manifested through communicative behavior in occasions of face-to-face interaction. In

other words, the task at hand is to ascertain first the dominant macrofunctions, then the dominant microfunctions of communicative behavior in occasions of face-to-face interaction. This paper will report only on the search for the dominant macrofunctions.

Data Pertinent to the Task at Hand

In keeping with the espoused epistemological orientation, the functions of communicative behavior are inferred from the various types of meaning attributed to this behavior by the members of the culture. Dominant macrofunctions are inferred from the types of meaning attributed to the entire range of communicative behavior without regard to the substantive differences that correspond to the various channels commonly distinguished, such as the linguistic, paralinguistic, kinesic, and proxemic channels.

In order to discover these meanings, a dual data base is utilized. It is comprised of (1) a specimen, i.e. a video and audio tape recording of a particular enactment of an occasion of face-to-face interaction or a portion of such an occasion, and (2) verbal accountings by members of the culture that reflect how they conceive of the communicative behavior displayed in the specimen. The latter are called folk verbal accounts. In a later phase of the analysis the behavior specimen is utilized as data for the investigation of the substantive manifestations of communicative behavior. In the initial phase of concern, as herein reported, the specimen is utilized only to provide an additional stimulus for the elicitation of folk verbal accounts.

Three types of folk verbal accounts are elicited: (1) recollections of the communicative behavior pertaining to the occasion of face-to-face interaction as a category of experience; (2) immediate recalls of the communicative behavior displayed via video recording of an actual instance of the occasion; and (3) commentaries generated during the viewing of the specimen.

The members of the culture serving as respondents are chosen on the basis of potentially relevant socio-cultural variables such as degree of in-groupness, sex, age, etc. So far the people participating in the specimen have been avoided under the assumption that they would be inclined to give self-conscious or idiosyncratic information.

Folk verbal accounts, as specified above, are data on the types of meaning attributed to communicative behavior by the members of the culture. The consideration of folk verbal accounts as data is characterized by two successive analytic steps: (1) the extraction of the types of information present in them, i.e. the native categories of information; (2) the interpretation of the significance of the latter for the task at hand. The ensuing presentation concerns itself with a discussion of the results yielded by these two steps.

Dominant MacroFunctions of Communicative Behavior

The folk verbal accounts obtained so far clearly suggest that normal, adult, middle class Americans organize their experience of communicative behavior in occasions of face-to-face interaction in

terms of two categories: as activities performed by people and as relationships between people.

That the members of the culture view communicative behavior as activities has the following implications. The activities performed by people in the course of an occasion characterize this unit as a cultural event. As a cultural event a particular occasion has a place within the entire life pattern of the members of the culture. It is one event of a certain type in a succession of events of various types. The function proposed to account for this way of organizing one's experience of communicative behavior in occasions of face-to-face interaction is called the event organizing function. This function provides the organizing principle for the activities taking place within a given occasion as well as for the various occasions, viewed as cultural events, that pertain to the life pattern of the members of the culture.

That the members of the culture view communicative behavior as relationships has the following implications. The relationships obtaining between people in the course of an occasion characterize this unit as the cultural frame within which the members of the culture enact their respective roles.(7) Relationships between people are interrupted but not necessarily terminated when an occasion ends. They may be pursued across similar or widely different occasions, occasions that are close in time or widely separated. Relationships, like activities, can be said to span the entire life pattern of members of the culture. The function proposed to account for this way of organizing one's experience of communicative behavior in occasions of face-to-face interaction is called the role organizing function. This function provides the organizing principle for the roles enacted within a given occasion as well as for the various occasions, viewed as background for role enactment, that pertain to the life pattern of members of the culture.

The event organizing function and the role organizing function are the two dominant macrofunctions postulated for communicative behavior in occasions of face-to-face interaction. To each of these functions corresponds a macrostructure. To the event organizing function corresponds the event structure of the occasion. To the role organizing function corresponds the role structure of the occasion. The event structure is comprised of the activities performed by the participants in the occasion. The role structure is comprised of the relationships obtaining between the participants in the occasion.(8)

The distinction between event structure and role structure has theoretical as well as operational implications. Theoretically this distinction is important because it relates to the ability of human beings to attend to more than one thing at a time. Thus in occasions of face-to-face interaction, normal, adult, middle class Americans attend to two things at the same time, the activities being performed and the roles being enacted. The unity of behavior reflects the particular way in which the two postulated macrostructures relate to one another: the event structure is the background for the role structure both literally and figuratively.

The event structure is, literally speaking, the background for the role structure since relationships are not enacted in the void but rather within a cultural event. The event structure is, figuratively speaking, background for the role structure in the sense that the former corresponds to the relatively stable and shared aspects of social life. In other words, the event structure represents the expected, the predictable, the obvious and consequently that which the members of the culture take for granted. In contrast, the role structure corresponds to the more fluid aspects of social life which are constantly negotiated and renegotiated. It is the unexpected, the unpredictable, what one worries about and consequently that which the members of the culture do not take for granted.

Operationally the distinction between event structure and role structure is important because it implies the order in which these two structures should be uncovered. The uncovering of the event structure is logically prior to that of the role structure for two reasons. First, the event structure serves to delimit in a non-arbitrary way the portion(s) of the entire occasion of face-to-face interaction to be investigated in depth. Event units that have clear-cut boundaries can be isolated for investigation without distorting the object of cognition. And second, since the event structure is viewed as a background for the role structure a prior knowledge of the former is necessary in order to gain knowledge of the latter.

III Conclusion

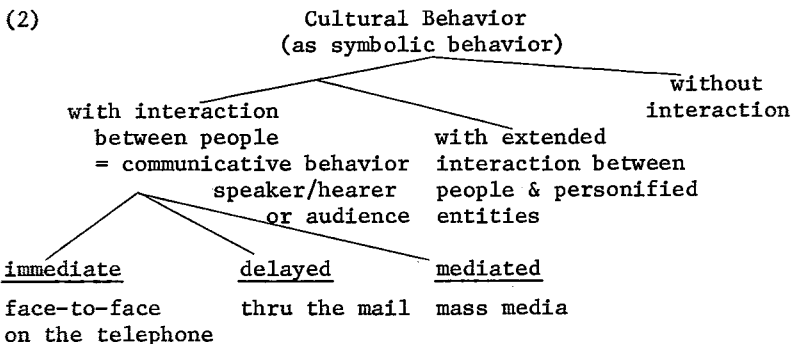
Our initial endeavor to develop a frame of reference for the analysis of face-to-face interaction can be summarized as to its analytic import in terms of the distinction Garvin makes between a theoretical model and a reference model. Notions with high potential general applicability pertain to a theoretical model. We suggest that the distinctions between macrofunctions and microfunctions and between macrostructures and microstructures constitute notions which belong to the theoretical model of our frame of reference. In other words, in the conduct of inquiry into any occasion of face-to-face interaction under any cultural circumstances, the questions relevant to these distinctions must be raised. The answers to the questions belong to a reference model; the answers being notions with more specific potential applicability. In raising the questions of function and structure with regard to data collected in the context of normal, adult, middle class American culture, we arrived at the notions of event organizing function, role organizing function, event structure, and role structure. We suggest that these notions belong to the reference model of our frame of reference and that within the specific cultural context from which they emerged they constitute culturally specific questions to further address to the data. It remains to be discovered as to whether the notions which have a place in our reference model for the investigation of occasions of face-to-face interaction in American middle class culture are also

relevant subculturally, cross-culturally, or even intergenerationally.

—

Footnotes

- (1) A more extensive version of this paper is to appear in *Semiotica*.



reflexive

soliloquy

- (3) For a fuller explication of the relationship between theory and method see Madeleine Mathiot, "Theory Building in the Descriptive Approach," in Method and Theory in Linguistics, edited by P. L. Garvin, Mouton & Co., The Hague, 1970, pp. 159-172.
- (4) This view is shared by David Schneider; see his definition of culture in American Kinship: A Cultural Account, Englewood Cliffs, 1968, p. 1 - "This book is concerned with American kinship as a cultural system; that is, as a system of symbols."
- (5) The very important question of the relationship between form and meaning, namely their co-variance is discussed by Paul L. Garvin in his inaugural lecture for the Fourth LACUS Forum, "An Empiricist Epistemology for Linguistics."
- (6) Note that this definition applies only to what Paul L. Garvin has called "external functions." For a fuller discussion of the distinction between external function and internal function see Garvin's inaugural lecture.
- (7) People's attitudes towards themselves and others reveal how they conceive of their respective roles. For an investigation that suggests how the notion of role can be operationalized see Madeleine Mathiot assisted by Marjorie Roberts, "Sex Roles as Revealed Through Referential Gender in American English," in Boas, Sapir, Whorf Revisited: Cultural Meaning in Grammar and Lexicon, edited by M. Mathiot, to appear in the Series Contributions to the Sociology of Language, edited

by Fishman, Mouton, The Hague.

- (8) Note that the focus on cultural activities as opposed to roles (and status) characterize much of cultural anthropology as opposed to small group sociology.

BILINGUAL STRATEGIES OF COMMUNICATION: EVIDENCE
FROM THE TEXT
David L. Lawton

Central Michigan University

The literature on bilingualism is extensive and covers a wide range of activities, but almost always from the perspective of oral performance. As used by Haugen (1956) and Weinreich (1966), 'bilingualism' refers to the acquisition of all other languages after the first, not simply to one other. Until the 1970s, discussions of bilingualism centered on the linguistic aspects of performance rather than on the social constraints limiting this performance. Recently, Fishman (1971) has drawn attention to bilingual societies in which the social correlates of language choice are important in determining which of two languages will be used in a given situation. Nevertheless, the perspective is that of the observer viewing oral rather than written performance. In this paper, I propose to examine the written performance of bilinguals as a strategy of communication in a clear departure from the classic view in which the bilingual is looked at as primarily an oral performer who keeps his languages neatly apart.

The bilingual who switches from one language to another is generally viewed as the kind of person whose needs are primarily informational rather than social and emotional. He keeps his linguistic systems apart by using the phonology and choice of idiom proper to each language. In some societies, however, such discreteness is rare. The bilingual will mix phonology, lexicon, and syntax according to social constraints and cross-over semantics. The bilingual carries over this oral switching to written switching within a literary context as a means of establishing social and intellectual rapport. This written form of the bilingual's production is neither discrete, for the most part, nor is it simply informational. Two societies in which the oral bilingual transfers his performance to writing as an art form are those of Puerto Rico and Jamaica. In each case, the written form provides a strategy of communication. Both societies are 'creolized' and both have extra-territorial members. Although 'creole' has not been generally applied to Puerto Rico (Lawton 1971), all the necessary conditions obtain. Both Puerto Rico and Jamaica have historical backgrounds in which slavery, language contact, and language domination have occurred. In the former island, the language milieu has been Taino, West African, Spanish, and English. The Taino was first; all the others were linguistic intruders. In the latter island, Arawak, Spanish, West African, and English; only West African and English merged

into a creole remain, together with English. In both islands the phenomenon of code shifting occurs, more pronounced in the large urban areas of Puerto Rico and the transatlantic barrio of New York City, in the case of Spanish and English, and in the city of Kingston and other towns in Jamaica, in the case of Jamaican Creole and Jamaican English.

Written strategy involves the use of single lexical items from English in a Spanish context, or from Spanish in an English context in the Puerto Rican situation; similarly, creole lexical items in an English context, and English lexical items in a creole context in Jamaica. The bilingual who transfers his oral skills to writing is speaking to other bilinguals in a bilingual/bicultural situation. He is reinforcing his identity, establishing his credentials, and communicating on several levels at once: informational, emotional, and sociocultural. Written strategy implies literacy. It also implies social awareness of the role language plays in a bicultural society. Yet the role of written performance as a strategy of social interaction has not been fully analyzed. By way of example, taking Puerto Rico first, look at 'About los Ratones' by Jorge Lopez (1975):

Los ratones venden las drogas
 la cojen--la usan--se meten las agujas
 sucias
 La usan en el bronx
 Se meten la coca como se meten en los clubs
 de billar
 They play nodding out pool
 They are behind the eight ball...

Note the statement of the topic in Spanish with the commentary in English. Both topic and commentary imply the restricted code (Bernstein 1972:164). Los ratones 'rats' has the extensional meaning 'drug dealers', which the audience would know. The situation itself has meaning to the bilingual audience establishing the credentials of the speaker/writer for both topic and commentary. The last two lines are brilliant in strategy because they capture not only the literal game itself, but also the metaphor of reference, 'behind the eight ball'.

The strategy of placing the topic in Spanish, keeping in mind that the 'speaker's' first language is Spanish, can also be observed in Martita Morales' (1975:49), 'The Sounds of Sixth Street':

Kids with innocent minds
 And their curiosity aroused
 '¿Mame, porque tú blanca y Papi tan?

Here, topic and commentary are reversed in time. The copula is deleted in the Spanish verse which would normally read, ' Mame,

porque tú eres blanca y Papi es tan? The deletion patterns with Black English and English-based creolized languages rather than with Spanish dialects, and may be a carry-over from Black English in contact with Puerto Rican Spanish. A detailed examination of the whole poem reveals the shifting back and forth between commentary and topic. In 'Twice a Month is Mother's Day' (Pedro Pietri 1975:124), the bilingual's use of the dramatic monologue makes code shifting the means of capturing the event and the mood of the topic in the barrio of New York City:

Mama looks real pretty today--even doña Olga
looks pretty--Mama says
'have to change el checke before the collectors
come around'
doña Rosa y doña María didn't get the checke
today--doña María she says she take the nenes
to the wilfredo oficina let 'em feed 'em
everyone says sí,sí,sí,sí,sí y que mas--cuz
they know she comes back for them--Mama says
she will speak for her tomorrow at the center
Mama she speaks very good english...

The bilingual's adaptation of two languages to written forms of discourse reflects in many ways his oral performance even to the recapturing of street rhythms. So far, however, the written forms have been poetic which, it could be argued, are essentially oral anyway. But the strategies that obtain in poetry also obtain in prose. In prose, the bilingual's performance often takes a satirical turn in which the bilingual himself may be the focus of satire. Take for example Miguel Santin's 'Trasfondo' (El Mundo, August 5, 1969):

Los Tres Santos Reyes? What's the que pasa with them?
Well, ahora nosotros tener un combo dirigido por
Santa Claus, who plays the electric guitar .. Ese
combo is the one that now distributes the juguetes
to the kids, you know?

Santin not only understands the function of the prose with interlocking Spanish and English, but he is able to poke fun at it. Just as Santa Claus can have a dual role, so can code-shifted Spanish/English in a bilingual, bicultural speech community.

The shifting or switching from one language to another in prose follows no statistically analyzable pattern except in the most obvious shifts. Shifting follows, rather, an intuitive perception of change of topic or rhythm, audience and place, in its written role. Oral situations in which shifting takes place are more subtly triggered and, sometimes, spontaneous. A glance or casual 'right word' is often sufficient motivation for shift. This skillful play with language which the bilingual possesses is sometimes revealed in what he does with the second language,

adapting it to first-language patterns by literal translation of idioms whose semantic points of reference are clear to the receptive bilingual. An example of this is the following:

I admit to you that I passed the fright of the cows
and for good time I could not dedicate myself to
the study of the English and more because in the
hospital I found myself with a nurse that was divine
and I invited her to give a turn around there and
drink a pair of little sticks on her day free.
(San Juan Star, April 23, 1968).

A native idiom such as un par de pequeños palitos 'a pair of little sticks' is the semantic equivalent of 'a few drinks'. In Puerto Rican Spanish, palito has the idiomatic meaning of 'drink' in the sense of 'shot of rum'. Other locations in the above which follow Spanish rather than North American English patterns are 'I admit to you', Le admito a Ud. The common English idiom, 'I was frightened out of my wits' becomes 'I passed the fright of the cows', Pasé el terror de las vacas. 'For a good while' becomes por buen tiempo; 'the study of the English', el estudio del inglés incorporates the article before a noun into translation to English where the context calls for article deletion.

It should be pointed out that the converse to the above also occurs; that is, the native Spanish speaker applies the same kind of transformation to Spanish in which English lexicon and phrasing are incorporated into Puerto Rican Spanish. Well known examples are such items as supermarketa 'supermarket', estinchí 'stingy', and levantar or leer las minutas 'to read the minutes of a meeting'. Extended passages incorporate formal but not semantic correlates from English as in

Por los últimos meses en Puerto Rico y en la
colonia Puertorriqueña dentro de los Estados
Unidos una discussion[sic] política se ha des-
arrollado... (Palante 1970: 2,10.).

Por for durante seems to be the literal translation of English 'for' although this point is open to dispute. The examination of texts in Puerto Rican Spanish where calques and literal translations from English occur always evokes the question and relevance of linguistic interference and confuses description with function. Arnaud Castel in L'effritement de la langue et de la culture espagnoles a Porto-Rico (1974) discusses the enormous change wrought upon Puerto Rican Spanish under contact with English but does not treat the function of Puerto Rican Spanish in its written guise. The point to keep clear is that the focus of this paper is on the bilingual's written guise. The Puerto Rican bilingual who commands both spoken and written forms of Spanish and English has a perceptive response to the use of

both languages in their functional mode. By 'command' I am including the relatively few persons who can keep the two languages apart as well as those who code-shift in varying degrees of intensity. Features incorporated from English into Spanish are less perceptible to the speaker whose first language is Spanish; hence, the conscious strategy of written usage is from Spanish to English where Spanish forms the matrix or base for the bilingual's commentary, his social satire, and his political anger.

The other ethnolinguistic situation in which the bilingual's role is adapted to the social context in subtle ways is that prevailing in Jamaica. Various scholars have expressed themselves on the nature of the linguistic melange in Jamaica (Alleyne 1971; Bailey 1966; Cassidy 1961, 1971; Craig 1971; DeCamp 1971; Lawton 1963, 1975, 1976; LePage 1961; LePage and DeCamp 1960) as to whether there are two languages or one, a continuum or no continuum, a creole or a post-creole dialect of English. More recently, Craig (1976) and Solomon (1976) have revived the discussion from the viewpoint of educational needs. In my view, Jamaica has two related languages as different from each other as Portuguese and Spanish: Jamaican Creole (JC) and Jamaican English (JE).

The constraints operating on the bilingual Jamaican's choice of JC or JE are broadly the same as those governing the performance of the Puerto Rican in choice of English or Spanish. There is a significant difference, however, in that JC has not been and is not now a prestige language. Quite possibly, the situation will change given the present political developments in which large numbers of the middle class are leaving the island; it is among the middle class where one finds bilinguals who can perform orally and in writing. In all the published research except one instance (Lawton 1976), no attention has been paid to the bilingual's strategy of using the written form of JC. The assumption is that JC has no written literature. Certainly this is not true although there is no separate writing system from English which would reveal the phonemic differences inherent in JC. The result is that inconsistent phonemic representation of JC compounds the problem of 'masking' where lexical items adapted to creole from English are represented in exactly, or almost exactly the same form.

One of the earliest examples of a written strategy is that in H.G. DeLisser's Jane's Career first published in 1914. The work is the first book-length novel of a Jamaican working girl in which the author, himself a bilingual, employs both JC and JE according to accepted social constraints. The use of two codes and the shifting from one to the other reveals a good deal about the class structure, the conditions under which one or the other code is used, and the cues which trigger each shift. Rather than use Jane's Career for illustrating the bilingual's strategy

in a creole society, I should prefer to use excerpts from Louise Bennett's *Jamaica Labrish* (1966), and Alan Mutabaruka's *Outcry* (1973). I shall not change the orthography to a phonemic script, but shall mark syllables for significant pitch: the acute accent for high, the macron for mid, and no mark for low.

Bennett's strategy is to take an everyday occurrence and put it into satirical perspective, using poetic form. The receptive bilingual reader automatically provides the correct phonemic form and the semantic component for forms homonymous with English.

Hairnét, scissōrs, fīne tēeth cōmb!
 Wēy de nīce lādy dēy?
 Buy a scissōrs from me nōh, lādy?
 Hair pīn? Tootpāse? Gōh wēy?
 Me sāy go-wēy āready, if
 yuh dōan like it sēe mē.

Bennett has committed an oral tradition to writing using her native knowledge of JC to appeal to the ear through the eye. The topic chooses the language. To dismiss Bennett as an entertainer (Nettleford 1966) is to fail to understand the bilingual's role as a cultural interpreter and critic. Bennett's writing in English rather than JC would be completely ineffective since there would be no meaningful connection between topic and language. She would have no audience since the topic and commentary would not ring true. The relatively few JE monolinguals or non-performing bilinguals would be unable to grasp the message. In JC, however, the message is clear even to those bilinguals who deny their bilingualism in a creole society.

Allan Mutabaruka, in 'Nursery Rhyme Lament', unlike Bennett, selects a well known nursery jingle rather than a contemporary event for satirical parody:

First tíme
 Jack and Jill
 Used to rún up dē hīll ēverydāy
 nōw dem gēt pípe ... an'
 water rāte incēase
 everydāy dem wōulda
 rēincārnāte hūmpty dūmpty
 fe fall off de wall ...

From the rather brief exploration of the bilingual's strategy in two ethnolinguistic situations, one can see that the bilingual in a bicultural society has both an oral and a written strategy. The written is generally neglected, perhaps, because an analysis of written production requires in-depth experience within the culture in such a way that the subtleties and nuances are intuitively perceived. In both poetry and prose, there is an increasing use of code shifted Spanish and English in the Puerto Rican community, and code-shifted creole and English in the Jamaican one.

References

- Alleyne, Mervyn C. 1971. 'Acculturation and Cultural Matrix of Creolization', in Dell Hymes ed., Pidginization and Creolization of Languages. (Cambridge U. Press)
- Bailey, Beryl. 1966. Jamaican Creole Syntax. (Cambridge U. Press)
- Bennett, Louise. 1966. Jamaica Labrish. (Jamaica, W.I.)
- Bernstein, Basil. 1972. 'Social Class, Language, and Socialization', in Pier Paolo Giglioli [ed.], Language and Social Contact. (Penguin), 157-178.
- Cassidy, F.G. 1961. Jamaica Talk. (Macmillan)
- _____. 1971. 'Tracing the Pidgin Element in Jamaican Creole', in Dell Hymes [ed.], Pidginization and Creolization of Languages. (Cambridge U. Press)
- Castel, Arnaud. 1974. L'effritement de la langue et de la culture espagnoles a Porto-Rico. Dissertation. (Universite de Paris-Sorbonne)
- Craig, Dennis. 1971. 'Education and Creole English in the West Indies', in Dell Hymes [ed.], Pidginization and Creolization of Languages. (Cambridge U. Press)
- _____. 1976. 'Bidialectal Education: Creole and Standard in the West Indies', International Journal of the Sociology of Language. Vol. 8, 93-134.
- DeCamp, David. 1971. 'Toward a Generative Analysis of a Post-Creole Continuum', in Dell Hymes [ed.], Pidginization and Creolization of Languages. (Cambridge U. Press)
- DeLisser, H. G. 1914. Jane's Career. Reprinted in 1971 by Rex Collins Ltd.
- Fishman, Joshua. 1971. Sociolinguistics. Second Printing. (Newbury House)
- Haugen, Einar. 1956. Bilingualism in the Americas: A Bibliography and Research Guide. American Dialect Society, No. 26. (University of Alabama Press)
- Lawton, David L. 1968. 'The Implications of Tone for Jamaican Creole', Anthropological Linguistics, 10:6, 22-26.
- _____. 1971. 'The Question of Creolization in Puerto Rican Spanish', in Dell Hymes [ed.], Pidginization and Creolization of Languages. (Cambridge U. Press)
- _____. 1975. 'Language Attitude, Utterance Recognition, and the Creole Continuum in Jamaica', University of Michigan Publications in Linguistics, Vol. 2, No. 2.
- Lawton, David L. 1976. 'Code Shifting in Jamaican Creole', New Directions in Creole Studies. Guyana, S.A.: Conference for the Society for Caribbean Linguists.
- LePage, Robert. 1961. Creole Language Studies II. (Macmillan)
- LePage, Robert and David DeCamp. 1960. Jamaican Creole. (Creole Language Studies I). (Macmillan)
- Lopez, Jorge. 1975. 'About Los Ratones', Nuyorican Poetry. (New York: William Morrow and Co., Inc.)

- Morales, Martita. 1975. 'The Sounds of Sixth Street', Nuyorican Poetry. (New York: William Morrow and Co., Inc.)
- Mutabaruka, Allen. 1973. 'Nursery Rhyme Lament', Outcry. Second printing. (Kingston, Jamaica: Swing Publishing Ltd.)
- Nettleford, Rex. 1966. 'Introduction to Jamaica Labrish, Louise Bennett', in Jamaica Labrish.
- _____. 1970. Palante, 2:10, August. (underground paper.)
- Pietri, Pedro. 1975. 'Twice a Month is Mother's Day', Nuyorican Poetry. (New York: William Morrow and Co., Inc.)
- _____. 1968. The San Juan Star, April 23.
- Santin, Miguel. 1969. El Mundo, August 5.
- Solomon, Denis. 1976. 'Foreign Language, Native Language, Standard Dialect: An Examination of Teaching Theory', Caribbean Journal of Education, 158-183.
- Weinreich, Uriel. 1966. Languages in Contact. Fourth printing from 1953. (The Hague: Mouton.)

SCHOLARLY AND LITERARY BILINGUALISM

Saul Levin

State University of New York at Binghamton

I

When we describe someone as bilingual, we usually mean that he has been heavily exposed to a second language and learned to speak it as fluently or almost as fluently as his mother tongue, or has even learned both at the same time in childhood. I would like to draw attention to a different kind of bilingualism, which may have an equal or greater effect in the long run. Where the main contact with the second language comes through books, learning depends upon deliberate instruction and study. Unsystematic exposure would result in practically no knowledge. For that matter hardly anyone learns to read his own language without pedagogy, just by being among people who are already in the habit of reading aloud or silently. Knowledge that begins with a series of lessons remains quite intellectual, long after it has been mastered and requires no conscious effort. For it has been acquired in the first place through awareness of elements.

Ever since the classic case of Romans studying Greek, the essential mark of this kind of bilingualism has been the superior standing of a certain written language. In Rome it was taught by Greeks, for several generations; but their role was ancillary to the masterpieces of poetry, oratory, and philosophy. The teachers were less important as representative speakers of Greek, from whom their Roman pupils would learn to talk to the kings, generals, and diplomats of the eastern Mediterranean. The Romans' interest in Greek did not diminish as they subjugated all that territory and proved their power to dictate terms to every Hellenistic government.

For the real prestige of the Greek language was secure from such temporal humiliations. It rested upon an incomparable literature from the past, because of which Greek books continued to be written by multitudes of more or less talented men, not all of them Greeks. As the Romans helped themselves to the riches of the world, so those Romans who cared to write well took the Greek classics for models, instead of the few, mediocre books that had come down in Latin.

To be sure, the Greek words came to their lips too, far from remaining shut up in their libraries except when studied. For it was the custom in antiquity to read books aloud;¹ so not just the items of vocabulary sank in but whole long phrases as well -- especially in meter. How much the leading men of Rome were impregnated with Greek cannot be measured but only illustrated by anecdotes. When Tiberius Gracchus, the reforming tribune, was assassinated after a rash political maneuver, his cousin Scipio Afri-

canus the younger quoted from the beginning of the *Odyssey* (1.47) Athene's comment on the murder of Aegisthus:

ὥς ἀπόλοιτο καὶ ἄλλος ὅτις τοιαῦτά γε ῥέξοι

'So perish whoever else may do the like.'² Augustus, on his deathbed, humorously recited the closing verses from a Greek comedy unknown to us:

ἐπεὶ δὲ πάνυ καλῶς πέπαισται, δότε κρότον

καὶ πάντες ἡμᾶς μετὰ χαρᾶς προπέμψατε

'Since it has been very well played, give applause, and all of you send us forth with joy.' To his wife he said something in Latin, perfectly traditional: *Livia, nostri coniugii memor vive ac vale* 'Livia, remember our life together; live and fare well' (Suetonius, *Aug.* 99.1). Julius Caesar had less time to think when he was stabbed, but some reported that he said a couple of words in Greek to Brutus: καὶ σὺ τέκνον 'you too, child' (Suetonius, *Iul.* 82.2).

The later emperor Marcus Aurelius made it the language of his heart, in which he wrote his personal *Meditations*. He must have learned it in childhood from a kindly tutor named Alexander (1.10); anyhow the expressions that recur in the *Meditations* are plainly the ones he picked up from his mentors in Stoic and Platonic philosophy. Latin was of course the main language of the men in power over the Roman empire, but they were more or less bilingual, depending on each one's interests.

By that time many masterpieces had been produced in Latin; but a Greek education remained primary for Romans long after it had ceased to be quite indispensable for acquiring literary skill. It was such an accepted custom that only the drastic impoverishment of the western Mediterranean countries in the fifth century of the Christian era pushed it out of the curriculum. By that time Latin itself, as taught in school, was far from the pupils' home language.

The condition then was diglossia rather than bilingualism. Whether it was still diglossia after the Romance languages developed, is a matter of definition. In Italy, as long as the Tuscan writers referred to their use of *la lingua volgare*, the term implied that it was fundamentally the same language as Latin -- just on a lower educational level.³ That is what scholars nowadays call diglossia. But in any case Latin coexisted similarly with non-Romance languages in Ireland, England, Germany, etc. In our terminology that is unquestionably bilingualism, although the very word *deutsch* or *Dutch* (originally *thiudisc* or *diutisc*) was a translation of *vulgaris* (or *vulgaris*) and referred first of all to the language of the common people ignorant of Latin; only later did it become the national name of that people.⁴

On the other hand the clerics, who had the most occasion to note the coexistence of Latin and Dutch (or another national language), did not think of Latin as the language of a foreign country but of the universal -- i.e., the Catholic -- church. That the church was centered in Rome aroused national jealousy at times; and in the age of Reformation the argument that the Latin liturgy is unintelligible to the laity was pressed by the Protest-

ant leaders most effectively in Germanic countries. Yet those leaders were accomplished Latinists themselves.

For a thousand years at least, when Latin was learned in school (or from tutors in the richest homes), it remained very much a spoken language among the educated minority. However, relatively few girls learned it. Grammatical drill was the main form of elementary teaching; but the teacher spoke Latin from the first, and the more clever pupils began to speak it too. The inattentive or forgetful ones got whipped; did the harsh discipline frighten many so that they mastered their lessons, or were the ones who learned docile and gifted by nature? I would hate to conclude that something so complex as a language can be methodically forced upon an unwilling mind.

In any case it is quite possible for the intellectual upper crust to know and use a language without being in touch with the native speakers, or after there are no native speakers left. It becomes somewhat creolized, as is bound to happen when spoken by those who have not enough access to the usage of fully competent speakers. The creolization of Latin was least extensive in morphology, since that could be taught quite systematically in school. But the morphology of another school language, Hebrew, was much affected by the Aramaic vernacular,⁵ because the teachers did not resort to a formal grammar for centuries after Hebrew had practically gone out of ordinary use.

A movement to restore the pristine quality of a school language and undo the corruptions of time is a recurrent phenomenon. The humanists of the Renaissance were most successful in recapturing the fine points of syntax, vocabulary, idiom, and rhetorical or poetic style. Latin phonology, however, eluded them in their study of ancient sources, for lack of a method to infer sounds from the mute evidence of the letters. At least they re-established the orthography.

Although they meant to increase the output of good Latin in every *genre* of prose and verse, they could not prevent it from succumbing ultimately. Indeed, one result of their emphasis upon the ancient classics, composed when the language was in its prime, was that the immeasurably greater bulk of subsequent Latin texts got consigned to a marginal status. While the humanists had a practical, prescriptive aim -- to delimit the authoritative models, whose Latinity should be emulated -- the implications have carried over into descriptive linguistics down to the present. For instance, although English is studied nowadays as a second language in much of the world, we linguists care little to examine the actual English usage of foreigners who have learned it at school. We note their deviations from the standard of the English-speaking countries, but we do not see the English of New Delhi or Tokyo or Stockholm as a language in its own right. Why should we, when there is plenty of native English to concentrate on? That is how the humanists felt about Latin.

II

A second language, if acquired by study rather than oral ex-

posure, has a somewhat different impact upon the native language. To be sure, the most obvious transfer, in either case, is of vocabulary items; but when the bilinguals make a practice of translating, instead of importing a word they will often calque -- i.e., express the meaning through native elements modeled upon something in the source language. Thus the Greek philosophical term συμπάθεια was translated *compassio* in Latin, and later *compassio* was translated *Mitleid* in German.

This sort of enlargement of vocabulary is least spontaneous. It does not arise on the spur of the moment, when the bilingual introduces a foreign word into a context where no native word for the thing exists or, if it exists, it does not occur to him. Calquing becomes frequent only through a deliberate policy, in the course of making translations or of restating or summarizing laws, doctrines, arguments, treatises for the benefit of monoglots.

The early Latin poets imitated the compound adjectives so frequent in Greek poetry but abnormal in Latin: thus *Iouis altitonantis* 'high-thundering Jupiter' (Ennius, fr. 541 Vahlen) from the Homeric Ζεὺς ὑψιβρεμέτης (*Il.* 1.354, etc.). But this sort of calque was largely rejected by the expert poets from Vergil on, as uncongenial to Latin. What caught on and proved permanent was developed by Cicero in his Romanization of Greek philosophy and rhetoric: *qualitās* for ποιότης, *quantitās* for ποσότης, *nōtiō* for γνῶσις, *perōrātiō* for ἐπίλογος, and dozens of others.⁶ Each Greek word is translated morpheme by morpheme, but the Latin creation falls into a long-familiar pattern exemplified by *cāritās*, *propinquitās*, *ratiō*, *pōtiō*, *perturbātiō*. The elements were there in the Latin language before Cicero; he was the one that sensed the need to combine them for the sake of intellectual discourse.

For oratorical training he was an inveterate translator from Greek (*De oratore* 1.34[155]). Most of his translations were mere exercises, which he never circulated; but he did publish his Latin version of Aratus' astronomical poems *Phaenomena* and *Prognoses* and of a section from Plato's *Timaeus*. As the Greek texts abounded in complex sentences, a simplifying paraphrase would hardly have done justice to their thought. But to find comparable resources within Latin syntax required ingenuity. The present active participle of most verbs had been rarely used in Latin; but it was now put to work to render the corresponding Greek participle. Other Greek participles, which found no close equivalent in Latin morphology, were handled in various ways such as a clause with *cum* and a subjunctive verb. Many more particulars have been investigated by Roland Poncelet in his massive book *Cicéron traducteur de Platon* (Paris, 1957).

The translation of holy scripture has always imposed an especially tight discipline, except perhaps in the twentieth century. Given that God was the source, no earlier translator would -- in principle -- take the liberty of rephrasing a verse as it might have sounded if originally revealed in his own language instead of Hebrew or New Testament Greek. Only when there was

something unintelligible was he bound to translate more independently. Otherwise a version might be either reasonably literal or extremely literal, but no version was free.⁷

For example, the Lord's prayer (one of the best known passages from any book) begins in Greek *πάτερ ἡμῶν* 'father of us,' which is an Aramaism; the genitive case of the pronoun is the standard, though unidiomatic, representation in Biblical Greek of the Semitic possessive suffix.⁸ In Latin it became *pater noster*, with an agreeing possessive adjective; that was the best equivalent to *ἡμῶν*. Idiomatic Latin had the vocative *mī pater* or *pater* alone (Terence, *An.* 139, 889, etc.), but not *noster pater*. So there was scarcely any temptation to change the order; *pater noster* would have been normal enough in a nominative function.

Among the subsequent versions made from the Latin (with or without reference to the Greek), the German is noteworthy for its perfectly literal *Vater unser*. English translators from the first have switched the order to *Our Father*.⁹ The principle of literalness prevails over the lack of vocative expressions with *our* in ordinary English, just as it did in Latin. But the plural possessive after *Father* was unacceptable, although the singular in that position is well attested in early modern English and still familiar in the quotation "O mistress mine" from Shakespeare (*Twelfth Night* 2.3.39).

Of the major European languages, German is the one whose syntax appears to have been most directly modeled upon the Latin. The stylistic preference of Latin prose authors, to bound a subordinate clause with the verb, engendered a fixed rule of German grammar. In English we have Dryden's more limited rule, also based on Latin, against ending a clause with a preposition.¹⁰ Good stylists before Dryden and since then have found that the pattern "For whom the bell tolls" comports a certain dignity if employed in a suitable context. Otherwise it is awkward and stilted.¹¹

The intricate style of the Latin poets, with the maximum interval between words that go together grammatically, had particular influence upon Italian verse; e.g., this quatrain consisting of one subordinate clause:

*quando suonar per l'aere,
infino allor s'è muto,
dolci s'udiro e flebili
gli accordi d'un liuto.*¹²

Rendered into English in the same order, it would sound grotesque, even incomprehensible: '... when to ring through the air, until then so still, sweet were heard and plaintive the strains of a lute.' The actual Latinisms of such a great poet as Milton -- e.g.,

Mee of these
Nor skilld nor studious, higher Argument
Remaines (*Paradise Lost* 9.41-43)

. . . to learn
What creatures there inhabit, of what mould,
Or substance, how endu'd, and what thir Power,

And where thir weakness, how attempted best,

By force or suttlety (2.354-358) --

enjoyed a vogue among lesser versifiers in the eighteenth century but then lost favor.¹³

The lasting effect from the experience of our English authors with foreign literature has been to discover or develop the equivalent to many of the niceties and complexities expressed in other languages. The Greek perfect or aorist participle τὸ παρεληλυθός or τὸ παρελθόν, with the neuter article, was rendered *le passé* in French. By the slightest adaptation this becomes in English *the past*, no longer construed as a participle or adjective except that it resists pluralization. But a more inventive rendering is, as Shakespeare put it, *what's past* -- treating *past* still as a normal adjective (e.g., *Hamlet* 3.4.150).

III

An influence conveyed through books is naturally upon the readers, and goes out from "Language A" to "Language B" without reciprocity. Seldom does it happen that two languages, both in the midst of a literary flowering, are mutually esteemed. English authors drew upon French for many centuries, copiously and with no hesitation. The converse is recent; and in literary French, as distinct from the "Franglais" of the street, the Anglicisms are still limited mostly to useful items of vocabulary.

French and Italian authors, to be sure, used to borrow quite a bit from one another; but in any one period the direction of borrowing depended on which culture was more advanced and innovative. In the age of chivalry, courtly love, the Norman expansion, and the Crusades, the Italian vocabulary was enriched by thousands of French words, with more or less regular adaptation to Italian phonology and morphology. Afterwards, during the Florentine Renaissance, educated Frenchmen looked to Italy. Not only did numerous terms come from there into French, but French grammar too was affected. Usage had not required the past participle to agree with the gender and number of the object; but the French grammarians (and grammatically minded writers) were unwilling to leave it optional and variable. They found in Italian a model for making the participle agree.¹⁴ It was actually not a strict rule in Italian, but no matter; the French grammars were able to insist on it, though only if the object comes before the participle. No pedantry would have availed to make a Frenchman say *Qui a *écrite cette lettre?* although in Italian *Chi ha scritta questa lettera?* is quite possible.¹⁵

The influence of Italy abated as French civilization on the whole regained the ascendancy by the end of the seventeenth century; but the Italian music masters and painters naturally went on introducing the terms of their arts into French and other languages through their pupils.

In the adoption of foreign words, scholars are noteworthy for adhering more methodically to certain patterns of adjustment, since they have studied the words and not just heard them. But a man of letters, however well read, is not necessarily a scholar;

Shakespeare is the supreme example of genius not worrying about formal niceties. Other English poets knew that the goddess of night and witchcraft is Hecate, in three syllables, like any such Greek feminine name coming through Latin as Latin was pronounced by Englishmen. But Shakespeare, in his reading, applied the English spelling rule of the silent final *-e* and made *Hecate* two syllables, sometimes spelling it *Hecat* or *Heccat*.¹⁶

Similarly, when he read about Disdemona and the Moor in Giraldo Cinthio's novella (*Hecatommithi* 3.7), the Italian text gave him no clue to the antepenultimate stress. He may have fallen into a false analogy with Latin names like *Pomona* or *Verona*, in which the penultimate *-o-* is long, and so pronounced it *Desdem'ona*.¹⁷ That he had picked up a reading knowledge of Italian is a great credit to him and to the liveliness of his *milieu* in London; for it was not taught in Stratford, and only by tutors anywhere in England, but anyone who had learned Latin and French well would have little trouble with it.¹⁸

For contrast with Shakespeare's casualness, take a case of scholarly concern that the original sounds should be carried over without alteration. Two Greek letters were added at the end of the Latin alphabet for the sounds [y] and [z], not native to Latin. The supplementary letters came in with the growing Hellenization of Roman culture, around the second century B.C. Before that, Romans were content to approximate an occasional Greek name or other word with the available Latin letters from A through X and, presumably, with the inventory of normal Latin sounds. But as they became more expert, they recognised the need to pronounce the Greek sounds and to write a word such as ΖΕΦΥΡΟΣ with Greek letters to the extent that Latin letters could not do justice to it. The Latin digraph PH was an adequate counterpart to the Greek Φ [p^h], but S would no longer do for the Greek Ζ nor V for the Greek Υ.

The later importation of the Greek accents and diaeresis into the Romance languages, especially French, illustrates scholarship in the service of a national orthography. Here it had nothing to do with the accurate rendering of borrowed Greek words. But the study of languages made the French humanists more aware of the defects in the customary (and variable) French spelling.¹⁹ Robert Estienne began (around 1534) by resolving the most prominent ambiguity, with the help of the Greek acute accent on participles such as *chargé*, to distinguish it from *charge*.

Whereas one Greek accent proved sufficient for Spanish, and pretty much for Italian too, the French scholars went on to discover needs or uses for all three accents -- the grave and the circumflex besides the acute. The poet Ronsard in 1550 applied the Greek rule associating the circumflex with long vowels, and so introduced *le nôtre*, *goût*, etc., where the vowel was long from the remote time that the consonant *s* had ceased to be pronounced.²⁰ The handling of the grave accent was more arbitrary. Corneille used it in 1660 to achieve a graphic distinction because a phonetic one existed in the language. He made it mark the more open vowel in *après*, *excès*, etc. (cf. the plurals *de-*

grés, glacés).

Such diacritics, which were gradually adopted by other Frenchmen in the course of the seventeenth and eighteenth centuries, made the makeshift Latin alphabet a little more workable in French.²⁰ A more drastic reform never took place.²¹ Further changes, to be of real use, would have had to discard the many silent letters that keep written French, even at present, much like Old French and somewhat like Latin in appearance, while the oral language has changed enormously.

Notes

¹G. L. Henderson, "Ancient Reading," *Classical Journal*, XXV (1929), 182-196; Bernard M. W. Knox, "Silent Reading in Antiquity," *Greek, Roman, and Byzantine Studies*, IX (1968), 421-435.

²Not surprisingly this report comes from the Greek historians Diodorus (44.7.3) and Plutarch (*Tib. Gr.* 21.7). The Latin authors can hardly be expected to mention it, since on the whole they did not presuppose bilingual readers.

³*Volgare* was itself not a word which had come down in the vocabulary of the masses, but a borrowing from Latin, with only a drastic simplification of the declensional endings; Carlo Battisti and Giovanni Alessio, *Dizionario etimologico italiano* (Florence, 1950-57).

⁴*Oxford English Dictionary*, s.v. "Dutch."

⁵Ze'ev ben-Hayyim, *Studies in the Traditions of the Hebrew Language* (Madrid, 1954), pp. 51, 59-61.

⁶See A. Meillet, *Esquisse d'une histoire de la langue latine* (repr. Paris, 1966), pp. 191-217.

⁷Sometimes the Aramaic Targum is a loose and expansive paraphrase of the received Hebrew text. That was possible because in origin it was oral and served in the synagogue to convey an approved or edifying interpretation of the Hebrew verse.

⁸In Homeric Greek ὦ πάτερ ἡμέτερε, with the possessive adjective in the vocative case, is part of a recurrent formula addressed to Zeus by Athene (*Il.* 8.31, *Od.* 1.45, 81, 24.473).

⁹*Ure fæder* in Old English; e.g., in Wulfstan's Homily 111a (p. 166 Bethurum). Prof. Paul Szarmach of Binghamton has called this to my attention.

¹⁰H. W. Fowler, *A Dictionary of Modern English Usage* (Oxford [1926]), s.v. "Preposition at end."

¹¹*Whom* survives in modern English only as a sort of Latinism; see E. Kruisinga, *A Handbook of Present-day English*, 5th ed. (Groningen, 1932), II, 2, pp. 170-171. Although of incontestably native and prehistoric origin, *whom* was bound to succumb to the reinterpretation of cases in colloquial modern English, which generalizes one case form (*me, us, him, her, them*), reserving *I, we, he,*

she, they for the role of subject in the immediate context of the verb (like *je, tu, il, ils* in French) and not following any verb except the auxiliaries *am, was, have, had, do, did, can, could, etc.* *Who* has been generalized to the elimination of *whom*, because *who* was always far more frequent both in questions and in relative clauses. *Whom* was not favored like *him* and *them*, because *whom* was never associated with a post-verbal position.

However, in literary usage *whom* is still inculcated wherever Latin syntax would call for an accusative, dative, or ablative case. The relatively few Latinists among us are practically the only ones able to "internalize" the rule about *whom* and to follow it unconsciously. Other educated people can pretty well learn to use *whom* after a preposition, since colloquially we are not at all inclined to say **To who were you talking?* but rather *Who were you talking to?* *Whom* at the very beginning of a question or relative clause goes more against the grain than *to whom, for whom, etc.*

¹² *Il Trovatore*, act I, scene 2 (libretto by Salvatore Cammarano).

¹³ See R. D. Havens, *The Influence of Milton on English Poetry* (New York, 1961), pp. 80 ff.

¹⁴ See Vaugelas, *Remarques sur la langue françoise* (Paris, 1647), pp. 176-181, citing the poet Marot.

¹⁵ From Vaugelas's example, "Premierement, le preterit va deuant le nom qu'il regit, comme quand ie dis, *j'ay receu vos lettres*. Alors *receu*, qui est le participe, est indeclinable, & voilà son premier vsage, où personne ne manque. Qui a jamais dit, *j'ay receües vos lettres*, comme disent les Italiens depuis peu, *ho riceaute le vostre lettere?*" it is evident that the present homophony of *requ* and *reques* has developed since his time.

¹⁶ E.g., *Midsummer Night's Dream* 5.1.377, *Macbeth* 3.5.1, *Lear* 1.1.

109. Trisyllabic *Hecate* only in 1 *Henry VI* 3.2.64.

¹⁷ As shown by the scansion of 1.2.25, 1.3.146, 292, 296, etc. *Disdemona* was actually formed by adding the Italian feminine ending *-a* to a base from the Greek *δυσδαίμων* - 'ill-fated.' No other character in the novella is given a name.

¹⁸ See M[ario] P[raz] in *The Reader's Encyclopedia of Shakespeare* (ed. O. J. Campbell; New York [1966]), s.v. "Italy in the plays."

A different possibility, though less likely, is that Shakespeare knew the novella only in Gabriel Chappuys's translation, *Cent excellentes nouvelles* (Paris, 1584; repr. by François-Victor Hugo, *Oeuvres complètes de W. Shakespeare*, V [Paris, 1868], 443 ff.) and so read *Disdemone*, which he could have Italianized with a final *-a* (instead of the French *-e*) while adhering to the automatic French stress upon the previous syllable, the last one with a full vowel.

¹⁹ Charles Beaulieux, *Histoire de l'orthographe française* (Paris, 1927), II, 22-23, 49-54, 64-68.

²⁰Estienne, however, injudiciously fastened a number of false etymological spellings upon the French language, such as *poids* 'weight' instead of *pois* or *poix*, as though it were from *pondus*; Beaulieux, I, 323. Since historical linguistics was in its infancy, he could not figure out that this French word is really from *pēnsum*.

²¹It was not attempted even during the great revolution, which modernized so many other customs. The leaders were extremely proud of their national language, and much concerned to spread it among the speakers of the *patois* and of Breton, Flemish, German, and Basque within the borders; Ferdinand Brunot, *Histoire de la langue française*, IX (Paris, 1927), 6-9, etc. They could hardly afford to see defects in the accepted rules of spelling.

CODE-SWITCHING IN WAR AND PEACE

Lenora A. Timm

University of California, Davis

I. Introduction.

In 1850, at the age of 22, Leo Tolstoy resolved to himself that he would turn over a new leaf: he would stop daydreaming and get on with the practical matters of life. To this end, he set forth a four-point program in which he included, among other things, the following stricture: "Ne pas sauter dans une conversation du français au russe et du russe au français" (Troyat 1965:82). Like most of Tolstoy's resolutions, this one was probably short-lived; but it serves well as an illustration of the proclivity for conversational code-switching on the part of the aristocracy of 19th-century Russia, the focus of today's paper.

Tolstoy has been widely acclaimed for the realistic depiction of characters and events in his novels; it is this fact, in addition to evidence from his own diaries and Souvenirs, which have led me to place considerable credence in the authenticity of many of the code-switching patterns discernible in War and Peace (hereafter noted as WP). Now, to be sure, Tolstoy not infrequently switches code as a literary device, sometimes for reasons which only he would be able to elucidate.¹ However, much of the code-switching in WP can be accounted for by principles which have emerged from recent works on bilingual speech communities--in particular, by viewing the switches in terms of "metaphorical" vs. "situational" motives (Blom & Gumperz 1972), and in terms of discourse configurations, built up from what Hasselmo (1970) has called "discourse markers" and "preformulated content units" (linguistic routines). Indeed, the fact that most of the code-switching discourse in WP lends itself to analysis along lines developed by those working with natural conversations of bilinguals contributes to the verisimilitude of the WP data. Moreover, Tolstoy interjects numerous metalinguistic comments in the body of the text which indicate his attentiveness to the linguistic usages of his characters (many of whom were based on real people in the author's life). For example, Hippolyte Kuragin's pronunciation (vygovor) is at one point described as that of a Frenchman who had spent about a year in Russia (V. I:29). Peter Nikolaevich Shishin is said to specialize in combining the "simplest popular Russian expressions with the most elegant French phrases" (V. I:77). Tolstoy explains Helen Bezukhov's switch to French during a mild altercation with her mother as due to Helen's feeling

that there was always some vagueness or lack of clarity (*neyasnost'*) when expressing herself in Russian (V. III:307).

Helen's comment is indicative of the nature of the bilingualism of some members of the Russian aristocracy, at least judging by Tolstoy's description of the situation--viz., that certain of them felt more confident in what was for several years the language of the country's military enemy than in their mother tongue. French was assiduously cultivated by most of the aristocracy, who tried at least to hire French-speaking tutors or governesses for their children, if not also to send them abroad for a portion of their education. Tolstoy himself knew the French alphabet as well as the Russian by the age of five, and somewhat later was able, according to his own accounts, to think directly in French (Troyat 1965:24). French was the language of *haute société* and of *haute culture*, and scions of aristocratic families could ill afford to be ignorant of it.

French seems to have been spoken daily, to some extent, in many upper-class households. However, individuals apparently varied in their preference for one language over the other, and in their proclivity for switching code.² Old Count Rostov, for example, is depicted as speaking very little French, apart from a few linguistic routines, such as greetings or terms of address. A minor female character in the novel, Marya Dmitrievna Akhrosimova, achieved her reputation as something of a social eccentric partially on the basis of her insistence on speaking Russian at all times; on the one occasion when she did speak French, it was, as Tolstoy explained, to "emphasize her slightly contemptuous and affectionate attitude" toward the addressee, Sonya (V. II: 714).

Then there were those upper-class persons whose Russian was so shaky that, during the years of anti-French sentiment which peaked in 1812, they felt impelled to hire tutors in their mother tongue. Tolstoy suggests in *WP* that social circles in Moscow agreed to speak only Russian; violators were to be fined and the funds deposited with a special committee. This committee must have been in possession of rather a fat kitty by the end of the year, for as socialite Julie Drubetskaya complained shortly after the rule went into effect: "Nel'zya govorit', kak skuchno!" ('It's impossible to talk, how boring!'; V. III:191). In spite of her exasperation, Julie attempts, in the same scene, to carry on in Russian, but soon resorts to French in giving it as her opinion that Princess Mary is "un petit peu amoureuse du jeune homme (Nicolas Rostov); when criticized for this violation by one of her male companions, Julie inquires ingenuously: "No kak zhe eto po-russki skazat'?" ('But how is this said in Russian?'; V. III:192).

Also of interest in Julie Drubetskaya's lapse into French in the above example is the *topic* which apparently promoted the switch. In another paper I intend to explore in detail topical and other extra-linguistic reasons for switching in *WP*, but suffice it to say here that among the most prominent topical motives

for switching to French in the novel is to make reference to courtship, flirtation, or other affairs of the heart; or for the description of womanly physical and/or "spiritual" attributes. In short, certain code-switching practices of the early 19th-century Russian aristocracy appear to justify that old adage to the effect that French is "la langue d'amour".

Structural aspects of code-switching discourse have captured the attention of a number of linguists in recent years. Blom and Gumperz (1972), Clyne (1972), Gumperz (1976), and Hasselmo (1970) have looked particularly closely at these matters, and my own thinking about the linguistic and psychosocial dynamics of code-switching has been greatly enriched by reaching their reports.

In the balance of this paper I intend to examine Russian/French (henceforth R/F) switching as represented in WP at the intrasentential level of patterning. I should point out here that the great majority of switches in the novel actually occur between sentences, sets of sentences, or paragraphs, but I must defer the analysis of higher-order switching to a later date.

In the next section of this paper I use the R/F data to test the validity of intrasentential constraints on switching as formulated in an earlier study of mine on Spanish/English (henceforth S/E) code-switching, and to consider the applicability of some syntactic constraints on switching recently expounded by Gumperz (1976). In Section III of the paper I turn my attention to those syntactic constructions which can and do occur in switched format in WP. In Section IV, I offer a few concluding observations.

II. Syntactic Constraints on Code-Switching.

In a prior study (1975) of S/E code-switching among California Chicanos, I tried to ascertain the syntactic limits on switching on the basis of several corpora of data, including a short story written by three Chicano students and an elicitation of responses by bilinguals to a number of switched sentences which I had devised. Briefly, the results I turned in were as follows: switching was considered impermissible (rated aberrant or bizarre by the bilingual judges) 1) between pronominal subjects or objects and the finite verbs to which they are immediately adjacent; 2) between finite verbs and adjacent infinitive complements; 3) within verbal constructions containing auxiliaries and main verbs (though there were a few exceptions to this in cases where the verb had been borrowed from English); 4) within negated verb sequences; and 5) within some N(oun) P(hrase)s containing one or more Adj(ective)s and a Det(erminer), the constraint here concerning mainly the order--English or Spanish--of the NP constituents and the lexical items chosen to represent them.

More recently, Gumperz has examined syntactic constraints on switching code among three bilingual populations--Spanish/English, Hindi/English, and Slovenian/German--, concluding that:

The ease with which a sequence can be switched is most closely related to the following factors:
 a) The length of the phrase or perhaps its stressability or contrastability [the shorter the phrase, the less likely a switch]...b) Sequential unity. Discontinuous sequences cannot be switched...c) Semantic or pragmatic unity. Idiomatic units cannot be broken. Conjunctions go with the phrase they conjoin...d) The total number of switches within any message subunit cannot be more than one (1976:35).

How do all these constraints jibe with the data from WP? In what follows, I will discuss my constraints 1-5 (in that order), interpolating remarks on Gumperz' constraints.

1) I found this constraint confirmed by the R/F data: there was no instance of switching between a verb and a contiguous pronoun. However, switching very occasionally occurred close to a V(erb)-Pro(noun) or Pro-V sequence:

- (1) dayu vam/ma parole d'honneur³
 (I give you/my word of honor)
- (2) otdaem yemu/chair à canon
 (we devote to him/cannon fodder)

In one sentence, the switch occurred between the Pro subject and the Pro object of the same verb:

- (3) ona/le mettait à la porte
 (she/showed him the door)

Gumperz has suggested that it is not necessarily a Pro, per se, which inhibits a switch before a V, but the brevity of the Pro, and its lack of stressability or contrastability (his constraint a), above). Cited as evidence in support of this idea is the marginal acceptability (to bilingual judges) of a longer and emphatic Pro such as that one in such a switched sentence as *That one/es el mas viejo, in contrast with the completely unacceptable **He/es el mas viejo (Gumperz 1976:32). This interpretation of my constraint 1 is attractive in some respects; on the other hand, example 3 shows us that a single, non-emphatic pronoun can stand alone before a switch, so it cannot be argued that the length or stressability of a phrase is always a decisive factor underlying switchability. Moreover, example 3 does confirm my first constraint, as originally stated; for additional confirming examples, cf. 61 and 62 below.

2) The second constraint in my S/E study blocked switching between a V and an adjacent infinitive complement; it seemed generally to be operative in the R/F case, for I located only two exceptions:

- (4) Napol'eon prikazal/ramener les popes /i...
 (Napoleon decreed/to bring back the priests/
 and...)
- (5) ot nevo zaviselo/verser/ili ne/verser le sang...
 (on him depended/to spill/or not/to spill the
 blood...)

In 4 there is a shift to French for the infinitive complement, then a return to Russian for the second clause of the sentence. In 5 the French infinitive complement occurs both affirmatively and negatively, with the negation achieved in Russian; the latter is a unique occurrence in the text, and an exception to the otherwise powerful constraint 4, which I will discuss shortly. The only explanation I can give for these two exceptional cases of switching is that they serve as a way of signalling indirect quotes. Now quotation is a well-documented reason for bilinguals' code-switching, and while it is an important motivation in WP, it is generally effected at the clause or sentence level; 4 and 5, therefore, remain somewhat aberrant.

3) The third constraint in the S/E corpus of data, which prevented switching between an Auxiliary) and V was strongly confirmed by the R/F data. One good reason for this, to be sure, is that such verbal sequences are not prevalent in Russian (since tense and aspect markers of the V carry out most of the functions of Aux). However, an interesting exception turned up in WP:

(6) Nikto ne byl/courtisée/kak ona
(No one was/courted/as she)

Now, while Russian syntax does allow past passive participles to follow the verb byt' ('to be'), it is impossible to express the particular meaning of 6 in this way; switching to French provided a quicker and pithier way of conveying the thought, compared with the Russian equivalent:

(6') Nikto ne byl predmetom ukhazhyvaniya kak ona
(No one was the object of courtship as she)

4) The fourth constraint discussed in the Chicano study prevented switching between negating elements and associated verbs. This check on switching was evidently also at work in the WP data, with the exception of sentence 5, above. The unexpected co-occurrence of a Russian negative with a French verb in 5 results, I think, from Tolstoy's switch to French to report what Alexander had written to Napoleon: that is, the letter was written in French, and presumably Alexander would have phrased it "...verser ou ne pas verser le sang de ses peuples", but Tolstoy here takes an author's privilege of stressing that which seems particularly crucial to him--i.e., not spilling the blood of his people; this emphasis is achieved by switching briefly to Russian. Thus, the use of code-switching for literary effect has, in the case of 5, crashed through one of the usual barriers to conversational switching.

5) The fifth and last restriction on switching in the S/E study related to NPs having one or more Adjs modifying the N, in addition to a Det. Limitations on switching here reflected, in part, the different typical ordering of Ns and Adjs in Spanish and English (Det-N-Adj for Spanish, Det-Adj-N for English), so that filling a pre-nominal slot, say, with a Spanish Adj preceding an English N resulted in an unacceptable NP (such as *su favorito/spot). On the other hand, shorter NPs consisting only of Det-N were likely candidates for switching.

The R/F case is analogous to this--i.e., Russian typically has Det-Adj-N ordering within NPs, while French has Det-N-Adj. The WP data contained no examples of internal NP switches which disrupted a Russian Adj-N sequence or a French N-Adj sequence. Of course, there are a number of French Adjs which may precede a N, and in such cases a switch did sometimes occur between a French Adj followed by a Russian N--e.g.,

(7) *ma bonne/Anna Mikhaylovna*
(my good/Anna Mikhaylovna)

(8) *ma chere/imeninitsa*
(my dear/name-day pet)

However, the most common type of 3-member NP showed a switch before a French N:

(9) *vse glupy/messieurs*
(all stupid/gentlemen)

(10) *nash milyy/cousin*
(our dear/cousin)

(11) *ta samaya/chair (à canon)*
(that same/cannon fodder)

In the following example, the switch occurs between a French Det and a Russian Adj-N sequence:

(12) *votre/vernyy rab*
(your/faithful slave)

Shorter NPs consisting of only Det-N turned out to be prone to switching, as in the S/E case; the majority of such switches in WP were from Russian to French, though a few were in the opposite direction:

(13) *dva/valets (de pied)*
(two/footmen)

(14) *svoikh/adolescentes*
(their/adolescents)

(15) *eto/suffisance*
(this/assertiveness)

(16) *kakoye/saute (au madère)*
(such a/saute (of Madeira))

(17) *mon/starosta*
(my/village elder)

(18) *des/donesen'ya*
(from the/reports)

Finally, a few other types of internally switched NPs occurred:

(19) *znamenityy/tête (de pont)*
(renowned/bridge-head)

(20) *gotovyashchiysya/mot*
(witty/word)

(21) *odna/petite personne*
(a/little person)

(22) *le cher/pravoslavnoe voinstvo*
(the dear/Orthodox army)

(23) *mon très honorable/Al'fons Karlych*
(my very honorable/Al'fons Karlych)

In reviewing examples 7-23, it can be seen that what might be called the basic syntactic integrity of the NP in either language is not affected by an internal switch--i.e., once a selection has been made for Russian or French Adj and N sequencing, only lexical items which normally occur in that sequence can be chosen to co-occur; sometimes lexical items from either language will fit a particular slot. Examples 7-23 also support Gumperz' statement that switching in bilingual discourse is limited to one switch per "message subunit".⁴

The examples and discussion so far have demonstrated that there are significant parallels between R/F and S/E constraints on intrasentential code-switching. The data in both cases have also, by and large, been supportive of most of Gumperz' conditions concerning the switchability of syntactic units.⁵ However, the WP data (and the S/E data) fail to support Gumperz' contention that "conjunctions go with the phrase they conjoin", as the following examples will illustrate:

- (24) evropeyskoe ravnovesie i/droit des gens
(European balance of power and/rights of the people)
- (25) Uspoysya i/parlons raison
(Calm yourself and/let's be reasonable)
- (26) vsë eto bylo samo modnoe i/comme il faut
(all this was very fashionable and/as it should be)
- (27) Peché veniel/ili/péché mortel?
(Venial sin/or/mortal sin?)
- (28) adolescents/i/adolescentes
(adolescents [male]/and adolescents [female])

Switching between a subordinating conjunction and following clause also occurred in several instances:

- (29) ty imenno dorozhish' tem chtob/ouvrir une carrière
(you especially value that for/opening a [new] field)
- (30) chustvovali, chto/le vin est tiré et qu'il faut
le boire
(they felt that/the wine is uncorked and it should be drunk)
- (31) rasskazyvali...o tom, kak/le médecin intime de
la reine d'Espagne/predpisal Elen...
(they told about how/the personal doctor of the Queen of Spain/prescribed for Helen...)
- (32) Razve ya ne vizhu, chto/du train que nous allons...
(Perhaps I don't see that/from the course that we're taking...)

It has been pointed out recently by Shaffer (1975:493) that switching between a subordinator and following clause has not been reported much in the literature on code-switching; he suggests as a reason for this the possibility that "embedding is a more close-knit construction than conjoining". However, these counter-examples (29-32), in addition to some documented in Clyne's study of German/English switching, cast Shaffer's proposition in a dubious light.

It seems clear from all of the R/F examples of conjunctions (24-32, and more could be cited) that the conjunction need not agree in code with the following phrase or clause. Indeed, a striking feature of the above examples is the exclusive use of Russian conjunctions, even in cases such as 27 and 28, in which the conjunctions are surrounded by French, quite possibly as a method of setting the two segments in contrast.

On the other hand, embedded relative clauses in WP tended strongly to have their relative pronoun agree in code with the rest of the clause:

- (33) un jésuite à robe courte/kotoryy dolgo v sadu...
(a short-frocked Jesuit/who for a long time in the garden...)
- (34) rebënka/qu'il avait sauvé des flammes
(of the child/whom he had saved from the flames)
- (35) dans le palais des Czars/gde dolzhny byli...
(in the Palace of the Tsars/where there should be...)
- (36) une petite personne/kotoraya ochen' neschastliva...
(a little person/who is very unhappy...)

Perhaps the bond between a relative subordinator and its clause is in fact more tightly knit than that between other types of subordinators or coordinators and the units which they embed or conjoin. Still, this bond did not prove inviolable for Tolstoy, for it was broken in one memorable trilingual sentence in WP, in which the contrastive effect achieved by using Russian subordinators (underlined) and mainly French clauses seems here purposefully intended:

- (37) Potom on rasskazal...epizodov iz/souvenirs d'Allemagne/gde/asile/znachit/Unterkunft,/gde/les maris mangent de la choucroute,/i gde/les jeunes filles sont trop blondes.
(Then he recounted episodes from/memories of Germany/where/shelter/means/Unterkunft,/where the husbands eat sauerkraut,/and where/the girls are too blonde.)

III. Options to Switch Code.

Though I have already given in Section II a hint of some of the options involved in switching within sentences (e.g., within types of NPs), there is considerably more material to cover in this regard. In this section, then, I will outline additional types of syntactic constituents which seemed relatively free to occur in switched configurations in WP.

Examples will be grouped under the following categories:

1) P(repositional) P(hrase)s; 2) NP or PP modifiers; 3) apposition; 4) predication; 5) VPs and VP Adverbials; 6) miscellaneous. Most of these groups will be subcategorized as I proceed.

1) PPs displayed a variety of internal switches, with the following three types being most prevalent in WP:

- P/NP
- (38) krome/vénération
(in spite/[of the] veneration)
 - (39) k/ma bonne amie
(to/my good friend)
 - (40) u/Annette
(at/Annette's)

- P Det/(Adj)(Adj)N
- (41) pro vashi/prouesses
(for your/prowesses)
 - (42) za eë/charmante soirée
(for her/charming evening)

- P Det Adj/N
- (43) du jeune/Bolkonski
(of the young/Bolkonski)
 - (44) s eë budushchim/directeur (de conscience)
(with her future/director (of conscience))

The majority of switches were in the direction of Russian to French; and the NPs within the PPs were subject to the same constraint on internal ordering as the NPs discussed in Section II.

2) Switching between NP or PP modifiers of other NPs or PPs was fairly common in WP. The following examples are representative of this pattern:

- (45) eta stolitsa/des Czars (this capital/of the Tsars)
(NP/NP)
- (46) dni/réunion (dans le palais des Czars) (NP/NP)
(days/[of] assembly (in the Palace of the Tsars))
- (47) pod imeniem/un homme (de beaucoup de mérite)
(PP/NP)
(by the name/[of] a man (of much merit))

The next example illustrates a more complex switch within a PP--i.e., a French PP is interposed as a modifying element between the Adj and the N of the head Russian PP:

- (48) s vzbitymi/à la grecque/buklyami
(with fluffed/Greek-style/curls)

Finally, I noted a couple of cases of Russian/German switching, both instances of NP/PP switches:

- (49) ataka/von diesem italienschen Herrn
(attack/by this Italian gentleman)
- (50) volnenie/des alten Herrn
(agitation/of the old gentleman)

(These last two examples are fragments of the thoughts of German-Russian bilingual staff members of the Russian army--Pfuel and Wolzogen, respectively.)

3) Another aspect of NP modification--apposition--occasionally occurred in switched configurations in WP; switching of this type allows, of course, for added emphasis or amplification of the head NP. In some cases the switched-to portion is a partial or full translation of the first NP (cf. 53 and 54):

- (51) muzh/grand seigneur
(husband/great lord)

- (52) Moscou, la ville sainte,/stolitsa
(Moscow, the holy city,/capital)
- (53) sosloviy,/états généraux
(estates,/States General)
- (54) plaki v kolësa--/les bâtons dans les roues
(spokes in the wheels--/spokes in the wheels)

4) Switching between subject and predicate (except where my first constraint is applicable) was one of the most common types of intrasentential switching in WP. I have noted at least 20 instances of it, in the majority of which the NP occurs in French, the VP in Russian, though not exclusively in that order.

Examples include:

- (55) Le charmant Hippolyte/porazhal
(The charming Hippolyte/amazed)
- (56) Le crème de la véritable bonne société/sostoyala iz
(The cream of really good society/consisted of)
- (57) skazal/l'homme de beaucoup de mérite
(said/the man of much merit)
- (58) starye devitsy/ont la manie des mariages
(old maids/are mad for matchmaking)
- (59) Directeur de conscience/byl izumlen
(The director of conscience/was astounded)
- (60) byla/Moscou, la ville sainte
(it was/Moscow, the holy city)

(The omission of the definite article in the French NP of 59 was, I think, quite deliberate: Tolstoy seems to be using this particular NP as a kind of title, with clearly ironic connotations.)

Switching between subject and predicate also took place when the subject (even a simple Pro) was followed by a verbless predicate (a common form of predication in Russian). I encountered several examples of this, including:

- (61) vy/un philosophe
(you/[are] a philosopher)
- (62) ona/un petit peu amoureuse du jeune homme
(she/[is] a little in love with the young man)

There were a number of sentences in which a switch occurred between a linking verb or participle and its predicative nominal or adjective:

- (63) eta byla/la grâce
(this was/grace)
- (64) vsë vykhodilo/charmant
(everything appeared/charming)
- (65) sdelavshis'/bas bleu
(having become/[a] blue stocking)

Occasionally the switch was located between a negative or an intensive and a following predicate nominal/adjective:

- (66) byt' tak/caustique
(to be so/caustic)
- (67) ona takaya zhe/pleurnicheuse
(she is such/[a] cry-baby)

5) Switches within VPs (V/NP) or between VPs and adverbials occurred over 20 times in WP. For both sequences, the direction

of the switch was predominately Russian to French, as the following examples illustrate:

- (68) imela/une femme de chambre
(she had/a chambermaid)
- (69) Pomnite/cette chère Annette?
(Do you remember/that dear Annette?)
- (70) vosstanovit'/les bons principes
(to reestablish/good principles)
- (71) ne prinimal/au sérieux
(did not take/seriously)
- (72) ne sposoben idti/sur les brisées de monsieur
(I am not able to go/in the footsteps of the gentleman)

Topicalization accounts for the reverse order of verb and object in the following example:

- (73) les femmes et le vin,/ne ponimayu
(women and wine,/I don't understand)

6) Bilingual sequences within this last, "miscellaneous", category can be sub-grouped into:

Expansions

- (74) no zdes',/dans ce vilain trou morave
(but here,/in this vile Moravian hole)
- (75) Pozdravlyayu, graf--/le baptême de feu
(I congratulate [you], count--/the baptism of fire)

Object complementation

- (76) nazyvala ego/mon page
(she called him/my page)
- (77) nazyvayu ego/Bonaparte tout court
(I'll call him/Bonaparte simply)

Interjections

- (78) Nu,/au revoir
(Well,/good-bye)
- (79) Une déesse!:/A?
(A goddess!:/Eh?)

Finally, I would like to note an instance of switching at the morphological level, resulting in a semi-integrated French loan:

- (80) schitayshiysya do sikh por/chef-d'oeuvre'/om
taktiki
(considered until then/a masterpiece/of tactical science)

The suffix -om tacked onto the French chef-d'oeuvre represents the instrumental case, required by this type of syntactic construction in Russian (the predicative instrumental). This was, so far as I could determine, the sole occurrence in the novel of switching at this level. (It is worth noting parenthetically that shedevr exists as an integrated loan in contemporary Russian.)

IV. Conclusions.

In this paper I have demonstrated that there are certain limits on sentence-internal switching between Russian and French, but that switching is, nonetheless, possible within various types of intrasentential phrases (including some NPs, PPs, and VPs) and among phrases within sentences (e.g., NP/VP, NP/NP, PP/NP, VP/Adverbial, etc.). That switching can occur so readily within sentences may dimly reflect the genetic affiliation of the two languages, but it more strongly suggests the possibility of a degree of rapprochement of Russian and French syntax in the minds of code-switching bilinguals.

Now, the existence of what may be called "compromise syntax" has been demonstrated for some other, very different, bilingual populations (cf. Clyne 1972 for German/English and Gumperz 1971 for Kannada/Marathi). Interestingly enough, Tolstoy himself seemed to have been aware of the tendency toward syntactic merger, for he pokes fun at it at least once in the novel, and he provides (perhaps subconsciously) a few examples of code-switching which can best be accounted for by assuming that certain syntactic configurations have to some extent been levelled or merged. The construction which Tolstoy himself comments on is udovol'stvie byt', as in:

(81) Chto za udovol'stvie byt' tak/caustique?

(What pleasure is there in being so/caustic?)

This is criticized as a "gallicism" (a calque), presumably modelled on the French plaisir être ('pleasure to be/in being'). Another example might be the following:

(82) kotoryy...delal/les honneurs/kraya

(who...did/the honors/of the district),

which goes one step further than 81 by switching to French within the VP, and which seems to be analogous to the French phrase faire les honneurs; the Russian equivalent of 82 is given in a footnote in WP (V. III:45, n.1) as:

(83) kotoryy byl zanyat delom priëma gosudarya

(who was occupied with the business of the reception of the sovereign).

More examples could be adduced, but I have run out of time. Let me say in closing that I have tried not to lose track of a third factor influencing the overall code-switching patterning discernible in WP--i.e., that one bilingual, Tolstoy, is ultimately responsible for it. Realistic though he may be as a novelist, it is certain that Tolstoy took some liberties in portraying the speech of his characters, and in using code-switching as a literary device, as I have occasionally noted in this paper. All in all, it may be said that Tolstoy left more than enough challenges to the student of code-switching, many of which I have barely touched on today, but to which I will return in the future.

NOTES

1. Indeed, Tolstoy was criticized by some contemporaries for being too liberal in language usage--e.g., for portraying Napoleon as occasionally speaking Russian, which was evidently contrary to fact. Tolstoy defended himself by alluding to the artist's license to exaggerate for aesthetic effect (1942:1354-1355). I might observe here that Napoleon's lines in WP are rendered far more often in French than in Russian; moreover, Tolstoy rarely depicts Napoleon as switching code within sentences, which is one of the chief conversational characteristics of many bilingual Russians in the novel.

2. One is likely to think here of the compound-coordinate distinction drawn in much of the psycholinguistic literature on bilingualism. Macnamara (1972) has, in my opinion, rightly criticized the distinction for its failure to specify clearly the nature of the putatively different semantic storage and retrieval systems associated with each type of bilingualism. The distinction has also been linked with different patterns of language acquisition, with respect to which some 19th-century Russian bilinguals would probably have been of the compound sort (both languages spoken in the home from a child's earliest months); others might be describable as coordinate bilinguals (French learned primarily from tutors, at school, or abroad). Be that as it may, if Tolstoy's sketches are accurate, code-switching was apparently practiced by both types of bilinguals.

3. In this and following examples, the diagonal line represents the location of the switch. The letter y in current American transliterations of Cyrillic may represent either the high central unrounded vowel [ɨ] (between consonants, finally, or before another y) or the semivowel [j] (elsewhere); the sequence yy represents the diphthong [ɨj]. Finally, an apostrophe after a consonant indicates palatalization.

4. Another interesting feature of switched NPs is gender coordination (or lack thereof) across the two languages. In most cases the Det and Adj agree in gender with the N. However, there are some exceptions, such as example 11, in which a feminine Det + Adj in Russian modify a French masculine N. A handful of similar cases of non-agreement occur in the novel; at least one of these was probably deployed to indicate the character's imperfect knowledge of French (Old Count Bolkonski at one point says eta/excellence, with a Russian feminine Det and a French masculine N); for the other examples of gender conflict, it is hard to say what Tolstoy had in mind, since the pertinent speakers elsewhere exhibited an excellent command of French.

5. Gumperz also exempts idioms from switching, which seems to me absolutely correct. I had already mentioned this in my S/E

study (1975:480, n.29); and WP provides many additional examples of the imperviousness of idioms to switching--cf., for just a few, 26 (comme il faut); 65 (bas bleu); 71 (au sérieux).

REFERENCES

- Blom, J.-P. and J.J. Gumperz (1972). Social meaning in linguistic structures: code-switching in Norway. In J.J. Gumperz and D. Hymes (eds.), Directions in Sociolinguistics: the ethnography of communication. San Francisco: Holt, Rinehart & Winston. 407-434.
- Clyne, M. (1972). Perspectives on language contact based on a study of Germans in Australia. Melbourne: Hawthorne.
- Gumperz, J.J. (1971). Convergence and creolization: a case from the Indo-Aryan/Dravidian border in India. In A.S. Dil (comp.), Language in social groups: essays by John J. Gumperz. Stanford, Calif.: Stanford University Press. 251-273.
- (1976). The sociolinguistic significance of conversational code-switching. In J. Cook-Gumperz and J.J. Gumperz, Papers on language and context. Berkeley: Language Behavior Research Laboratory (Working Paper, 46).
- Hasselmo, N. (1970). Code-switching and modes of speaking. In G. Gilbert (ed.), Texas studies in bilingualism. Berlin: Gruyter. 179-210.
- Macnamara, J. (1972). Bilingualism and thought. In B. Spolsky (ed.), The language education of minority children: selected readings. Rowley, Mass.: Newbury House. 60-76.
- Shaffer, D. (1975). The place of code switching in linguistic contacts. In P. Reich (ed.), The second LACUS forum. Columbia, S.C.: Hornbeam. 487-496.
- Timm, L.A. (1975). Spanish-English code-switching: el porqué y how-not-to. Romance Philology 28(4). 473-482.
- Tolstoy, L.N. (1942). War and peace. Trans. from the Russian by L. and A. Maude. New York: Simon & Schuster.
- (1960). Voyna i mir. 4 vols. Moscow: Gosudarstvennoe uchebno-pedagogicheskoe izdatel'stvo.
- Troyat, H. (1965). Tolstoï. Paris: Fayard.

RELATIONAL NETWORK APPROACHES TO CODE-SWITCHING

The Second Foundation*

The National Institute of Mental Health
and
Michigan State University

0. Introduction

Although the subject of code-switching is receiving increasingly greater attention from researchers in sociolinguistics, bilingual education, and social psychology, the exact meaning of the term remains elusive and ill-defined. Witness the following statements [our emphasis]:

...code-switching is the use of *more than one language* by communicants in the execution of a speech act. (Di Pietro 1976:1)

"Code-switching" has become a common term for alternative use of *two or more languages, varieties of a language, or even speech styles*. (Hymes 1974:103)

We define code-switching as the use of *two or more linguistic varieties* in the same conversation or interaction...The varieties may be anything from *genetically unrelated languages to two styles of the same language*. (Scotton and Ury 1975:1)

We accept the rather broad definition of code-switching given in the quote from Scotton and Ury; at this point, however, it is probably moot whether or not we precisely define what is meant by a *variety* of a language. The important point is that communicating individuals perceive some linguistic alternations as taking place within the same subsystem, whereas they perceive others as taking place across subsystems. Experimental studies have shown, moreover, that it is possible to identify when a code-switch occurs, what factors trigger it, and what it signifies in terms of social meaning.

For example, Blom and Gumperz (1972) discovered two types of code-switching in a Norwegian village. One, *metaphorical switching*, was used to convey a speaker's attitude toward the topic of the conversation, while the other, *situational switching*, was used to convey a speaker's attitude toward his audience. Similarly, a study by Scotton and Ury (1975) showed that the use of different languages among multi-lingual residents of Kenya served the purpose of changing the social distance between the participants in a conversation.

In this paper we are going to discuss code-switching within a relational network theory of language, more specifically that known as stratificational theory (Lamb 1966, Lockwood 1972). In previous papers (The Second Foundation 1976a, 1976b, 1977) we have investi-

gated the possibility that descriptions of human social behavior can be stratified into a *sociotactics* and a *transactional tactics*. The sociotactics specifies the relatively stable and long-lasting social relationships which exist between individuals and groups of individuals; the transactional tactics specifies organized units of behavior that individuals in a variety of social relationships can participate in, and which may also require a rapid shift of roles. For instance, a parent engaged in conversation with a child may be the speaker one moment and the listener the next, but the social relationship /parent-child/ remains constant.

1. Groundwork for a Model of Code-Switching

Several recent advances in stratificational theory have been incorporated into our model of social behavior, and we would like to briefly discuss them before presenting the relational network analyses of several code-switching phenomena. Herrick's work on the treatment of "style" in stratificational theory and Johanneson's work concerning the representation of knowledge in the individual's cognitive system will be discussed here, while the theory of network activation proposed by Dell and Reich (1977a, 1977b) is discussed in Section 4.

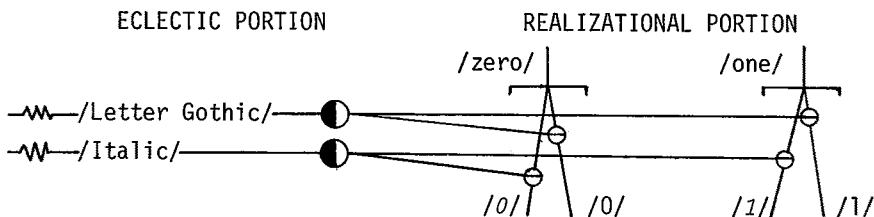
We may consider code-switching as belonging to a wider range of phenomena which have been discussed in stratificational theory under the name "style."¹ As early as 1968, D. Bennett wrote that

stylistic questions--more specifically questions of 'register'...--such as the fact that a given lexeme may be restricted to colloquial, scientific, etc., varieties of the language, are probably best handled by setting up a separate subsystem of the grammar...in such a way that the choice between two items represented in the main network as being in free variation could be shown to be determined by the choice of register. (1968:166-167)

Herrick has expanded and refined Bennett's notion of a separate subsystem into what he calls the *eclectic portion* of a language.

The idea behind the eclectic portion of a language is that it intersects the realizational portion of that language wherever stylistic alternations occur, its function being to control such alternations during encoding and interpret them during decoding. Figure 1 shows a simple example of stylistic variation controlled by the eclectic portion.

Figure 1: Control of stylistic alternation by the eclectic portion.

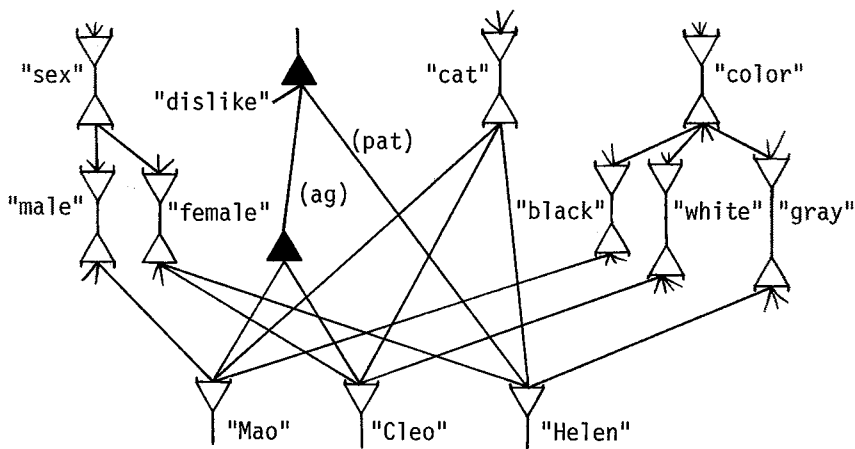


The language used in this example is a simple binary language having two concepts, /zero/ and /one/. The written form of these concepts may appear in one of two typefaces, /Letter Gothic/ or /Italic/; a change in typeface does not involve a change in the "meaning" of these marks--which we will not discuss here--only a change in their graphic shapes. The choice to use a particular typeface is made outside the eclectic portion but is projected into it to eventually control the alternate realizations of /zero/ and /one/ through the small, circular *GATE* nodes in the realizational portion. The wavy lines indicate omitted structure which will not concern us in this paper.²

It is becoming obvious that an understanding of human communication requires an understanding of cognition and memory.³ Johannesson's work on the English modals (1976) presents what is probably the best account within stratificational theory of the structure of the cognitive system and its relationship to language.

Johannesson's conception of the cognitive system is essentially a series of interlocking taxonomical hierarchies; Figure 2 shows a portion of a hypothetical individual's knowledge concerning three cats: Mao, Cleo, and Helen.⁴ Inspection of the figure will show

Figure 2: A portion of a hypothetical individual's cognitive system. (Adapted from Johannesson 1976)



that Mao is a black male, Cleo is a white female, and Helen is a gray female. The shaded nodes represent a proposition which could be glossed as "Mao and Cleo (the agents) dislike Helen (the patient)."

Our first example of code-switching will show how sociotactic information can be integrated into this type of representation of the cognitive system.

2. Markedness in a Relational Network Analysis of Code-Switching

The concept of markedness has proven to be valuable in describing many forms of sociolinguistic behavior; in this context, markedness refers to the restriction of a language variety to cer-

tain situations or to its use to emphasize certain qualities of either the speakers, the situation, or both. A familiar example of marking is the use of *tu* in French or *du* in German to mark intimacy between the speaker and hearer.⁵ A less familiar example is the use of English in some multi-lingual African societies to mark the speaker as an educated person; in contrast, the use of an ethnic language which is not the speaker's own indicates that the speaker identifies himself with the native speakers of that language, or would like to. In cases where a speaker would like to remain neutral by not marking himself or the situation with certain language-linked qualities he may be forced to code-switch from language to language to achieve this neutrality (Scotton 1976).

In this section we will present a relational network analysis of some of the behavior which was observed taking place among the population and staff of a nursing home, behavior which exhibits the marking of certain transactions on the basis of sociotactic information and the subsequent realization of such marking through the use of several varieties of American English.⁶ The data which we will consider are as follows:

1. The Residents of the nursing home (R's) always use a variety of speech which may be called /informal/.
2. Among themselves, the Aides of the nursing home (A's) use /informal/ speech; with an Outsider investigating the nursing home (O) they used a style of speech which may be called /consultative/; and with the Residents, they use a speech style which may be called /downtalk/.⁷

Our treatment of this data will make use of a notational device which indicates preference of certain choices over others; this device is known as an *ordered OR*. In conjunction with other nodes such as *DIAMONDS* or *upward ANDs*, we may use *ordered ORs* to explicitly show what factors determine a choice. In Figure 3, for example, both diagrams may be translated as "In the presence of X, take path B; when X is not present, take C instead."⁸ Note that it is not necessary to mark the factors which determine the choice of path C; they are simply understood to be all those factors not included in X.

Figure 3: The use of *ordered ORs* to indicate markedness.



Figure 4a shows the relevant sociotactic information for this data; we will consider this data primarily from the point of view of an Aide in the nursing home, although it happens that the representation shown here is applicable to a Resident or to the Outsider as well.⁹ Figure 4a also shows some of the connections of the so-

Figure 4a: Sociotactics for the nursing home data.

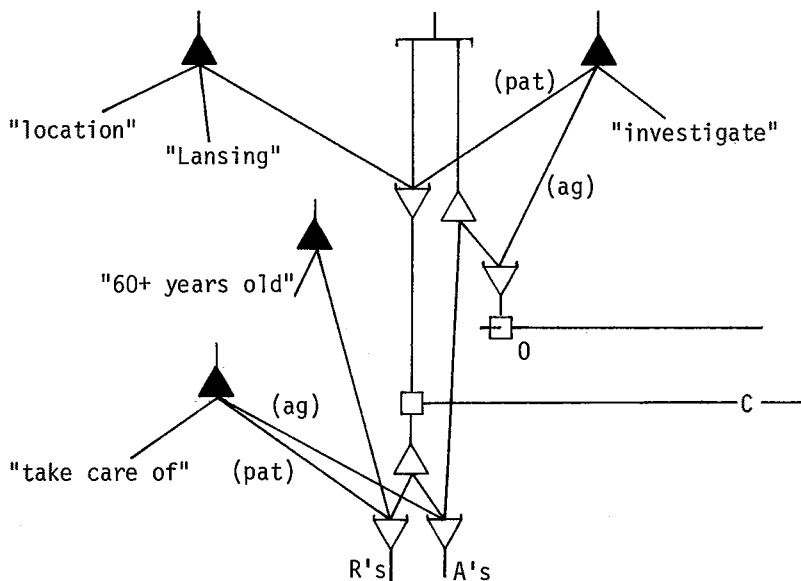
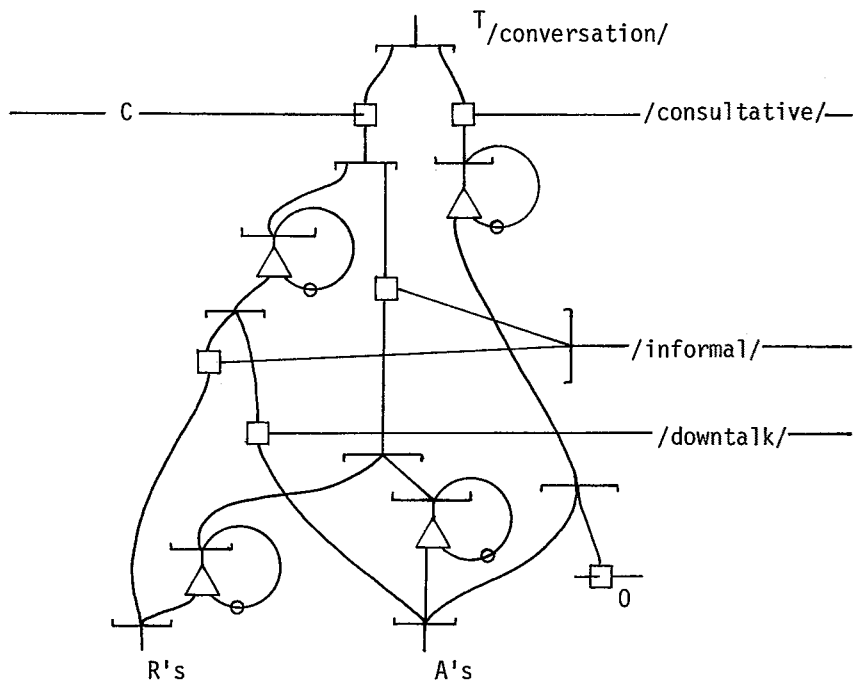


Figure 4b: Transactional tactics for the nursing home data.



ciotactics to the rest of the cognitive system, shown in shaded nodes. For example, an Aide knows that the nursing home is located in Lansing and that it is also under investigation by the Outsider. Two propositions shown at the lower left represent the facts that Aides take care of Residents and that Residents are over 60 years old.

Notice that the Outsider is related to the Aides via the right-hand side of the *ordered OR* at the top of the diagram; this captures the fact that the Outsider-Aide relationship is less marked, the more marked construction being reached via the left-hand path of the *ordered OR*. This construction represents the social group made up of the nursing home staff and population. The line C leads down from the sociotactics into the transactional tactics to control the selection of linguistic varieties in conversations.

Figure 4b shows the transactional tactics needed to account for our nursing home data.¹⁰ The unmarked situation is represented by a line leading down from the right-hand path of the topmost *ordered OR* to a looping construction which represents a conversation type in which Aides talk with the Outsider; the *DIAMOND* dominating this structure indicates that all participants in such conversations use the style /consultative/. If a conversation involves members of the nursing home population or staff, the left-hand path of the topmost *ordered OR* must be taken. As mentioned above, line C determines this choice. In this situation, there are two types of conversations, one involving speech between "equals," that is, between Aides or between Residents. In such conversations all participants use /informal/ style. The second, more marked, conversational type occurs when an Aide speaks with a Resident; in this case the Resident will use /informal/, but the Aide uses /down-talk/.

Our analysis makes certain predictions about situations not given in the data; if, for example, an Outsider were addressed in /informal/ style by an Aide, he would probably interpret it as a sign of acceptance by the Aides. If, however, he were addressed in /down-talk/ he would most likely feel that the Aide had been rude in treating him in the same way as a Resident.¹¹

3. Solidarity and a Displaced Code-Switch

Many studies of code-switching have focused on the use of a particular linguistic variety as a marker of *solidarity*, by which we mean that "feeling of togetherness" which occurs between individuals (or groups) when the commonalities between them are shared or emphasized. In this section, we will discuss an instance of code-switching to indicate solidarity and also present a tentative explanation of *why* the linguistic varieties in question were used. The datum to be considered appears in Figure 5.

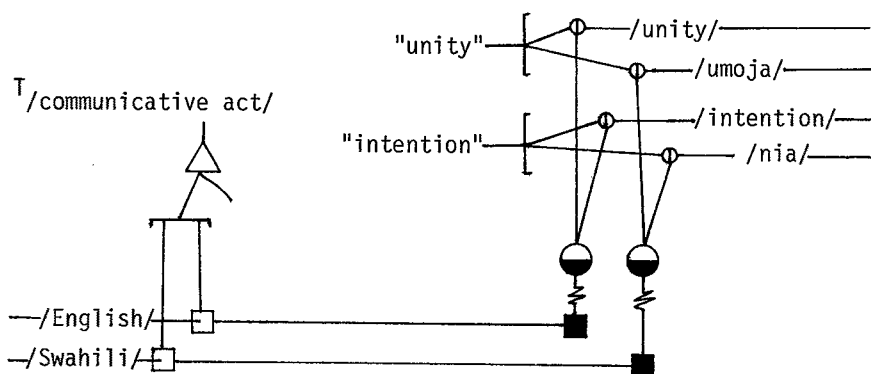
A poster of this design appeared on the bulletin board of a dormitory at Michigan State University; the languages involved are English and Swahili, a rough translation being "Unity. Good will is our intention." We consider this poster as an instance of a *displaced code-switch*, a code-switch presented to observers in

arity/ represents the mapping of real-world experience onto the abstract conceptual system. At the present we cannot specify how certain physiological activities--perceived in terms of "feelings," "needs," and so on--are mapped onto the cognitive system, thus activating the strategy /solidarity/. Neither can we specify the reverse process whereby the perception of certain concepts expressed in language or actions are felt to satisfy the desire to feel solidarity with others. Hence the question marks in Figure 6a. The concept /solidarity/ is related to various other concepts, among them /Swahili/.

Note that it is possible to trace a path from "ego" to /Swahili/ to "African" and back to "ego." This closed loop is shown in dotted lines in Figure 6a; in this example, we believe that it is the concept "African" which is "filled in" by the designer/perceiver to close the loop between "ego" and /Swahili/, thus emphasizing a common attribute.

Figure 6b illustrates the markedness of Swahili in a transaction labeled T/communicative act/ for convenience and the subsequent conditioning of Swahili alternates in the linguistic system through an eclectic portion.

Figure 6b: Transactional and eclectic systems for discussion of solidarity.



4. Code-Switching in a Model of Performance

One of the goals of stratificational theory is to develop a model of performance which is compatible with a relational network theory of competence. Since code-switching is a dynamic activity, it is only fitting that that we discuss a possible performance model in this final section.

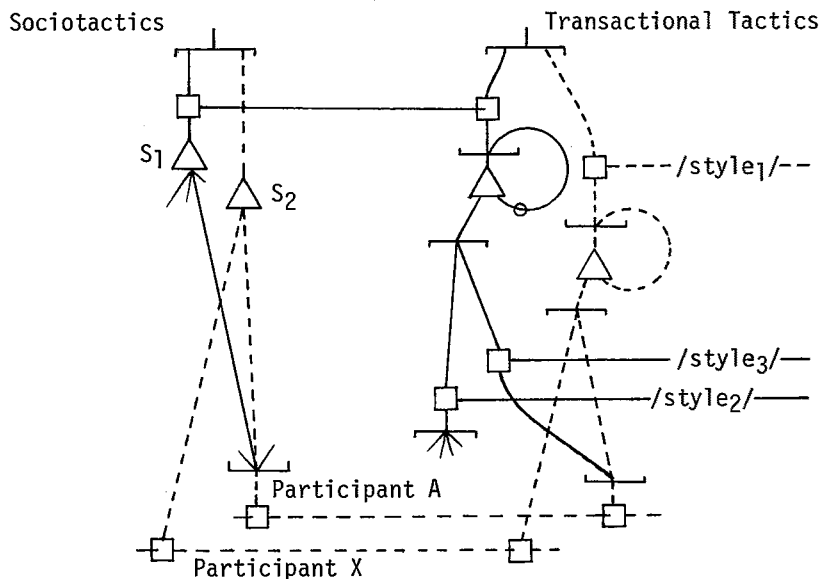
There are several competing formalizations of network activation in current stratificational theory (Christie 1976, Reich and Dell 1977); the one which we favor is that proposed in Dell and Reich 1977a and 1977b. They present a system of *spreading activation* in the network, whereby the activation level of a node is dependent on the activation it receives from nodes immediately connected to it; furthermore, the amount of activation a node trans-

mits depends on its activation level. Although we cannot go into the details of this system here, we can at least sketch how the concept of spreading activation might be useful in developing a performance model of code-switching in a relational network theory.

Figures 7a and 7b show the network representation of an abstract situation in which a code-switch takes place. Figure 7a represents part of the knowledge that Participant A has of the social and transactional structures of his society. For example, at the sociotactic level, A's knowledge includes information to the effect that he belongs to a number of social constructs, one of which is a marked relationship labeled S_1 . The other lines leading out from S_1 connect to persons whom A knows participate with him in that relationship.

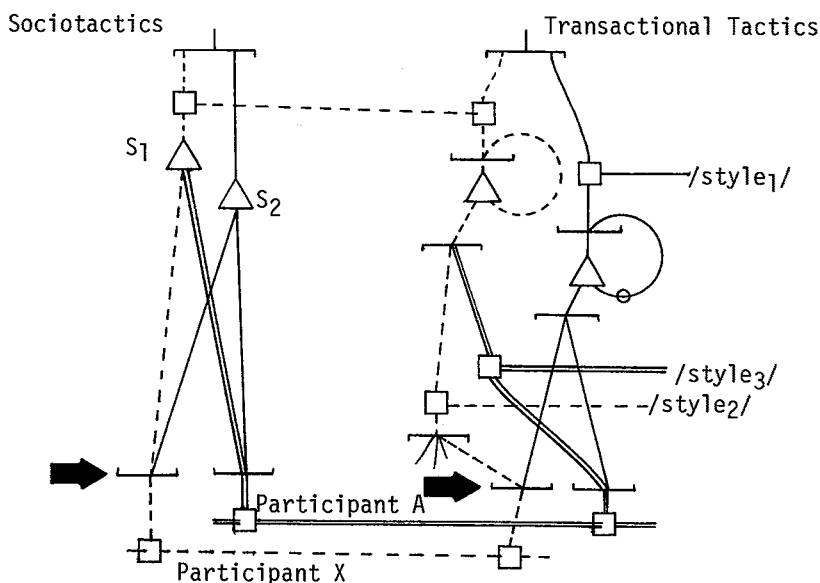
A's only shared connection with Participant X is through the unmarked social construct S_2 , however, which requires the use of /style1/ by both participants in a conversation, as shown in the transactional tactics. The dotted lines in Figure 7a indicate the dominant flow of activation in this situation; the solid lines indicate "passive knowledge," and they receive much less activation.

Figure 7a: The dominant flow of activation before a code-switch.



In Figure 7b, let us assume that for some reason Participant X has just code-switched from /style1/ to /style2/, alerting Participant A that X has changed his perception of their social relationship. The dotted line in Figure 7b shows the resulting shift in the flow of activation in A's cognitive system, a shift which results in the creation of two new nodes--marked by arrows--and the subsequent connection of X to the social construct S_1 . If A accepts the attempt by X to redefine their relationship, he can signal this by increasing the activation flow along the path indicated

Figure 7b: The dominant flow of activation after a code-switch.



by the double lines, which will result in his code-switching from $/style_1/$ to $/style_3/$.

5. Conclusion

It is hoped that the foregoing discussion of code-switching will generate further investigations into the application of relational network theories to sociolinguistic phenomena; several future avenues of research suggest themselves. We have not touched upon the fact that code-switching probably also entails differences in kinesic as well as linguistic behavior. It might be possible to use silent movies of code-switching behavior to discover if the switches can be identified by kinesics alone. The investigation of "closed loops" in the cognitive system, such as that described earlier for our solidarity example could lead to a general theory of how one concept becomes a symbol for another. Also, as our knowledge of the structure of the cognitive system increases, it may eventually be possible to computer simulate code-switching behavior. We hope to discuss some of these points at a future LACUS Forum.

NOTES

- * We would like to thank Mr. Robert Distler, Jr. for reading our paper at the 1977 LACUS Forum held at McGill University in Montreal, Quebec, Canada. Comments concerning our work will be gladly received at either of the following addresses:

Michael Eric Bennett
Department of Linguistics
Michigan State University
East Lansing, MI 48824

Dr. Elizabeth W. Nail
Mental Health Study Center
National Institute of Mental Health
2340 University Boulevard East
Adelphi, MD 20783

1. For a comprehensive discussion of the meaning of "style" in this context, see Herrick 1977.
2. See Herrick 1977 for details concerning the eclectic portion and its operation.
3. See, for example, Vipond 1975 and Phillips 1976.
4. We prefer to use *AND-ORs* in the cognitive system rather than the simple *ORs* used in Johannesson 1976; this difference is not crucial to any points made here.
5. The situation is not this simple; for further discussion see Brown and Gilman 1960 and Ervin-Tripp 1972.
6. This data is based on a study done by Martha M. Rhodes of the Department of Linguistics, Michigan State University.
7. For a discussion of /informal/ and /consultative/, see Joos 1962. The variety which we call /downtalk/ is characterized by a sing-song intonation pattern and over-careful pronunciation.
8. For further details concerning notation, see Lockwood 1972; a familiarity with this work is assumed throughout this paper.
9. Rigter (1976) correctly notes that in spirit, stratificational linguistics deals with idiolects; for individuals who share the same culture and language, however, the overlap in idiolects is great enough to allow a high degree of abstraction. Also of interest to points made here is Feinbloom's (1976) observation are aware not only of the behavioral options which are open to them, but also those that are not.
10. For purposes of clarity, connections of conversational structures to text-generating structures are not shown; neither are the connections of the various speech styles to the eclectic portion.
11. It might be possible to test some of these predictions using tapes in the manner described in Scotton and Ury 1975.
12. For further discussion of strategies, see The Second Foundation 1976 and Di Pietro 1976. In this discussion, the "ego" is assumed to be Black. The concept of "shared information" is most likely at the root of all *In-Group/Out-Group* distinctions, and the confusion felt by members of the Out-Group (in this case, Whites and other ethnic groups who were not aware that Swahili was the other language on the poster) may be related to processing difficulties in the cognitive system when they attempt to find the "missing link."

REFERENCES

- Bennett, David C. 1968. "English Prepositions: A Stratificational Approach," *Journal of Linguistics* 4, 153-72.
- Blom, J.-P. and J. J. Gumperz. 1972. "Social Meaning in Linguistic Structures: Code-Switching in Norway." In *Directions in*

- Sociolinguistics* (J. J. Gumperz and D. Hymes, eds.). New York: Holt, Rinehart and Winston.
- Brown, Roger W. and A. Gilman. 1960. "The Pronouns of Power and Solidarity." In *Style in Language* (T. Sebeok, ed.). Cambridge: M.I.T. Press.
- Christie, William. 1976. "Evidence Concerning Limits on Central Embeddings in English," *Forum Linguisticum* 1.25-37.
- Dell, Gary S. and Peter A. Reich. 1977a. "A Model of Slips of the Tongue." In *The Third LACUS Forum* (R. J. Di Pietro and E. L. Blansitt, Jr., eds.). Columbia, S.C.: Hornbeam Press.
- , 1977b. "Toward a Unified Model of Slips of the Tongue." MS.
- Di Pietro, Robert J. 1976. "The Strategies of Language Use." In *The Second LACUS Forum* (Peter A. Reich, ed.). Columbia, S.C.: Hornbeam Press.
- Ervin-Tripp, Susan. 1972. "On Sociolinguistic Rules: Alternation and Co-occurrence." In *Directions in Sociolinguistics* (J. J. Gumperz and D. Hymes, eds.). New York: Holt, Rinehart and Winston.
- Feinbloom, Deborah Heller. 1976. *Transvestites and Transsexuals: Mixed Views*. Delacorte Press/Seymour Lawrence.
- Herrick, Earl M. 1977. *Extra-linguistic, Non-semological Conditioning of Categorical Alternation*. Unpublished Ph.D. dissertation. Michigan State University.
- Johannesson, N.-L. 1976. *The English Auxiliaries: A Stratificational Account*. Stockholm: Almqvist & Wiksell International.
- Joos, M. 1962. "The Five Clocks." *International Journal of American Linguistics* 28, pt. V.
- Lamb, Sydney M. 1966. *Outline of Stratificational Grammar*. Washington, D.C.: Georgetown University Press.
- Lockwood, David G. 1972. *Introduction to Stratificational Linguistics*. New York: Harcourt Brace Jovanovich.
- Phillips, Brian. 1976. "Structure in Discourse." In *The Second LACUS Forum* (Peter A. Reich, ed.). Columbia, S.C.: Hornbeam Press.
- Reich, Peter A. and Gary S. Dell. 1977. "Finiteness and Embedding." In *The Third LACUS Forum* (R. J. Di Pietro and E. L. Blansitt, Jr., eds.). Columbia, S.C.: Hornbeam Press.
- Rigter, Bob. 1976. Review of Lockwood 1972. *Linguistics* 180.80-92.
- The Second Foundation. 1976a. "Transactions and Realizations: An Overview." In *University of Michigan Papers in Linguistics*, Volume 2, Number 2.
- , 1976b. "A Cognitive Model for the Social Construction of Reality." In *The Second LACUS Forum* (Peter A. Reich, ed.). Columbia, S.C.: Hornbeam Press.
- , 1977. "A Cognitive Model for the Social Construction of Reality: Life and Language in Contemporary American Society." In *The Third LACUS Forum* (R. J. Di Pietro and E. L. Blansitt, Jr., eds.). Columbia, S.C.: Hornbeam Press.
- Scotton, Carol Myers. 1976. "Strategies of Neutrality: Language Choice in Uncertain Situations," *Language* 52.919-941.

- and William Ury. 1975. "Bilingual Strategies: The Social Functions of Code-switching," *International Journal of the Sociology of Language* (to appear).
- Vipond, Douglas. 1975. "Understanding Ambiguous Sentences." In *The First LACUS Forum* (Adam Makkai and Valerie Becker Makkai, eds.). Columbia, S.C.: Hornbeam Press.

CODE SWITCHING AND THE PROBLEM OF BILINGUAL COMPETENCE*

John M. Lipski

Michigan State University

The phenomenon of code-switching in bilingual speakers is of great significance to both linguists and psychologists; to the former, because it offers an insight into the workings of the underlying systems of language, and to the latter, because it permits further investigation into the question of the harmful or beneficial effects of bilingualism. Most studies of code-switching to date have focused on psychological/sociological aspects, or on elementary linguistic constraints of a very general nature. Often, code-switching is forced in unnatural situations (e.g., Kolers 1966) and the data are of questionable validity. The present study seeks to fill in some of the gaps in the investigation of code-switching, by dealing with purely linguistic constraints which appear to apply in cases of English-Spanish bilingualism in the United States. No attempt will be made to assess code-switching in terms of lack of lexical availability, since it has already been demonstrated (e.g., Lance 1975, Clyne, 1967, Hasselmo, 1970) that code-switching is possible even where both variants are available. Due to limitations of space, this note will be confined to a survey of typical examples of Spanish-English code-switching, with an eye toward formulating a working hypothesis governing the types of inter-linguistic constraints that the bilingual might use. These considerations in turn have implications for the much broader question of the linguistic competence of the bilingual speaker. In addition to observations by the present writer, the examples used come from the following sources: Espinosa (1917), Gumperz and Hernández-Chavez (1970), Lance (1975), Redlinger (1976), Timm (1975), Valdés Fallis (1976).

Gumperz and Hernández-Chavez (1970) note that whole sentences are most easily borrowed, followed by sentence modifiers or phrases. They do posit, however, the impossibility of phrases like:

- (1) *que have chamaquitos (who have kids)
- (2) *he era regador (he was an irrigator).

In the first type, tentatively put on the prohibited list, the relative pronoun in Spanish is followed by an English verb and then a Spanish complement. It must be admitted that this type of switched expression does sound rather unlikely, but the issue is complicated by the fact that the regional colloquial word chamaquitos (or, for that matter, any other commonly-used noun

that might be put in its place) is frequently used in English, and thus might not be properly considered as exclusively part of the Spanish code; in such a case, the format would be, instead of SES, SEE, which is a type-form that does occur in spontaneous Spanish-English bilingual speech. The second case discussed above involves an English subject pronoun followed by a Spanish predicate. The bond between subject pronoun and verb is normally an indissoluble one in both English and Spanish, although the same does not hold for proper nouns, which may be switched freely.

Timm (1975) established several constraints on code-switching. The first category, similar to the one mentioned above, deals with switching of pronominal subjects or objects:

(3) *yo went, *ellos gave, *mira him, *dijo to them

with English word order and:

(4) *him mira, *to them dijo, etc.

with Spanish word order. Very few exceptions to this tendency have been noted, thus suggesting this as a cardinal rule governing code-switching.

A second restriction involves switching between a finite verb and an infinitive complement:

(5) *(they) want a venir, *quieren to come, etc.

The SSE sequences were judged universally inappropriate, while the sequences EES:

(6) he wants to bailar

were considered possible, although not common. Timm speculates that such a sequence is marginally permissible because "the E verb and preposition (or particle) seem to function as a sub-unit within the larger construction" (478). An alternative, or perhaps equivalent view, is that the Spanish infinitive contains a clear morphological suffix (-ar, -er, or -ir) and is thus susceptible to insertion in a phrase which demands an infinitival complement without introducing ambiguity. The English verb, in contrast, is not morphologically marked in the infinitive, and, thus, inserting such a verb in a Spanish phrase demanding an infinitival complement creates an ambiguous and bizarre construction.

In verb phrases containing auxiliaries, code-switching is normally prohibited:

(7) *(I) must esperar, *(he) has visto, *debo wait, *estaba walking, etc.

It was noted, however, that expressions such as they were chope-

ando, he was cachado, etc. were possible, utilizing in each case an English loanword which had been morphologically adapted to fit Spanish paradigms. One does, however, occasionally hear exceptions to this general rule; for example Redlinger (1976: 47) notes the example mi marido está working on his Masters.

Negation of verbal elements also poses an obstacle for code-switching, the normal rule being that the negative words must be in the same code as the verb being negated, thus impossible sequences would be:

- (8) *I do not/do no/not/quiero, *I no want, *I not quiero, etc.

Finally, within noun phrases, there is considerably more flexibility as regards the possibilities of code-switching, although certain restrictions do appear. In phrases of the type D + N, switching occurs virtually without restraint, unless involving stereotyped combinations of noun and determiner, but once adjectives are involved, the situation becomes more complicated. The following D + A + N or D + N + A combinations were judged as unacceptable: English word order:

- (9) (SSE) *su favorito spot, (SES) *su favorite lugar,
(ESS) *his favorito lugar, (ESE) *his favorito spot;

Spanish word order:

- (10) (ESE) *his lugar favorite, (EES) *his spot favorito,
(SES) *su spot favorito.

The English-ordered EES sequence

- (11) his favorite lugar

was considered acceptable, while SEE

- (12) su favorite spot and ESS his lugar favorito

were, while less than totally acceptable, judged at least possible. It should be noted that in all such phrases, it is possible to interpret some of the elements as borrowed phrases, even though it is less likely to borrow a combination of noun and adjective. Even in some of the cases judged generally unacceptable, informants questioned by the present writer disagreed with each other, often adducing the same potential interpretation as containing borrowings; for example, su spot favorito, etc.

So far, the only general rules about code-switching are those prohibiting splitting main verbs from auxiliaries, subjects from verbs and adjectives from nouns and appear, in most cases, to be manifestations of some more general constraints, which dictate that, superficial morphological format notwithstanding, certain phrasal elements are atomic and, consequently,

unbreakable; verb + auxiliary and certain prepositional phrases appear to fall into these categories. It is perhaps significant that, for example, in the evolution of the Romance languages from Latin, there has frequently been interchange between synthetic single-word forms and analytic multi-word forms, with precisely the same semantic values; this alternation has involved such categories as the future, conditional, present and pluperfect forms as well as various subjunctive forms, also the value of certain prepositions. Prepositional phrases, too, appear to be in many instances regarded as being atomic entities, and are used as such in the function of adjectives, adverbs, and sometimes even nouns; this might help explain the extreme reluctance with which code-switching occurs within the boundaries of a prepositional phrase. The splitting of articles from nouns, except in cases of borrowed forms, appears also to violate a fundamental principle of linguistic structure, especially in Spanish. While the English article merely identifies the noun and supplies a varying degree of definition, depending upon whether a/an or the is chosen, the Spanish article adds the further information of gender and number. In most cases, the Spanish articles are redundant check-morphemes which serve to enhance the grammatical information already present in the morphological makeup of the noun, although in certain cases gender and/or number cannot be predicted from the superficial form of the noun. The fact that as many as three morphemes may unite the noun with the article in Spanish may well account for the extremely tight bond which normally exists between article and noun, a bond which usually resists attempts at code-switching. In a similar fashion, the bond between subject pronoun and conjugated verb is a natural one, especially in English, where the subject position, due to the slight degree of differentiation among verbal forms, may never remain empty. In English, either a pronoun or a noun must occur in subject position, while in Spanish, the subject position may, and, in the case of non-human nouns must, remain empty of a pronominal subject. Therefore, at least in English, each finite verb is automatically regarded as connected with a corresponding subject pronoun, in the absence of a noun; it may be this fundamental difference in the obligatory versus optional status of the subject pronoun between Spanish and English that accounts for the reluctance to code-switch between subject pronoun and finite verb or this restriction may simply be a factor of the indissoluble bond in English, which does, in fact, exist in Spanish as well, at a deeper level of structure, since every finite verb must have a subject at some point in the derivation.

The reluctance to switch codes in infinitival constructions also may be a function of differing morphological systems: the Spanish infinitive is clearly marked with the final r, while in English, the preposition/particle to, used before the dictionary-form of the verb, is considered to be the "sign of the infinitive." Thus, the English substantival infinitive is an analytic construction whereas the Spanish infinitive is a synthetic form;

mediation between these two paradigms is particularly difficult, all the more so since in English the sign of the infinitive precedes the stem form, while in Spanish the infinitival morpheme comes at the end of the stem. It is, therefore, not surprising that little, if any, code-switching occurs in infinitival constructions, except where the entire verb has been borrowed from one language to the other. It is interesting to note that when English borrows a Spanish verb, normally the entire infinitive is taken, to which is prefixed the particle *to*: *to bailar*, *to romper*, etc. Spanish, on the other hand, normally takes the English dictionary-form and suffixes either *-ar* or *-ear*: *watchear*, *workear*, *mopear*, etc.

In all of the investigations carried out to date, the search for linguistic environments for code-switching has taken place in a unilingual setting; that is, one looks for features in the preceding (or occasionally, following) linguistic context, in most cases material from a single language, to predict the occurrence or non-occurrence of a switch. Alternatively, one speaks only in terms of abstract grammatical categories, such as noun phrases, prepositional phrases, etc. in assessing cases of code-switching, which automatically places that analysis at some level of deep structure at which both languages share a common structure. Few investigators who have dealt with the problem of code-switching have really gone beyond the boundaries of the individual language, although in many cases a purely surface-structure analysis has been performed. This appears to stem from the failure to distinguish between two distinct linguistic systems, as present, for example, in a bilingual dictionary, and the systems as present in the cognitive structures of truly balanced bilingual speakers. It is highly unlikely that the two languages are stored in a person's competence in the tabular form found in bilingual dictionaries or word-lists; the very fact of code-switching and interference seem to imply at least a certain amount of interconnection. If, therefore, one works from the hypothesis that bilingual competence is in fact a form of interlanguage, in the sense of Selinker (1969, 1972), it then becomes possible to examine instances of code-switching not only with regard to unilingual environments, but also as forming part of a truly bilingual, or interlingual set of surface structures.

Since the ways in which two distinct languages can mix to form an interlinguistic interface are virtually infinite, the possibilities for discovering a principled interaction between the two languages in the speech patterns of bilinguals is equally formidable. Nonetheless, an overview of the available data suggests at least some general directions which may be explored. Most significantly, it appears that, whatever the sufficient conditions may be, the necessary conditions for bilingual code-switching involve a certain form of homology between the superficial syntactic form of the utterances in the two languages. Put in other words, this means that the overall superficial form of a given utterance *X* must be essentially the same in *L*₁ and *L*₂, with

respect to the general arrangement of constituents and the deployment of the bonds of semantic structure in order to switch from one code to the other in the midst of the production of the sentence. In its strongest form, such a hypothesis would take the following form:

- (A) Given an (underlying) semantic representation S , and given the superficial representations X_1 and X_2 of S in the languages L_1 and L_2 , respectively, in order for a code-switch to occur in the production of S , it is necessary that the superficial syntactic structures of X_1 and X_2 be identical.

The establishment of such a hypothesis is, of course, impeded by an accurate definition of syntactic equivalence; nonetheless, in most cases, working rules may be established, even if a comprehensive definition of syntactic similarity or identity remains elusive. Hypothesis (A) could, of course, be successively weakened by replacing the word "identical" with "essentially similar," which in turn generates a host of new problems concerning the meaning of "similar." However, once again, since code-switching appears under the best of circumstances to be an idiosyncratic phenomenon, such an addition should not be regarded as theoretically objectionable, however difficult it may be in practical terms.

Consider the sentence

(13a) *Terminé la escuela in the Navy*

with the main subject-predicate clause in Spanish (I finished school) and the prepositional phrase in English. Sentence (13a) would, if presented totally in Spanish, be roughly

(13b) *Terminé la escuela en la Marina*

while in English, the phrase would become

(13c) *I finished school in the Navy.*

A point-by-point comparison of the syntactic structures of the three sentences reveals a nearly perfect congruence among the three in terms of the placement and semantic functions of the superficial constituents. The only points of divergence are the optional use of the subject pronoun in Spanish, a topic which has already been discussed, and the use of the definite article *la* with *escuela*, and its omission in English. The placement of the article is dictated by rule in Spanish, and in fact the entire phrase *terminé la escuela* has a nearly fossilized format, in which native speakers of Spanish would automatically insert an article without further thought. Thus, a code switch involving either (13b) or (13c) could theoretically occur at any point in the production of one of the sentences without changing the over-

all superficial form of the final product; the only restrictions would involve specific intra-sentential bonds of the sort discussed earlier. Sentence (13a) can be further extended by the addition of a subordinate clause, as in

- (13d) I finished school while I was in the Navy
 (13e) Terminé la escuela mientras estaba en la Marina.

Again, the two expanded sentences have the same superficial structure, and code switches, as judged by native speakers, are also permissible in phrases such as

- (13f) Terminé la escuela mientras I was in the Navy/while I was in the Navy
 (13g) I finished school while estaba en la Marina/mientras estaba en la Marina.

Of these four possibilities, only the first variant of (13g) seems a bit anomalous, perhaps because Spanish requires no subject pronoun in the subordinate clause (since the subject is the same as that of the principal verb) while English demands a pronoun in this position; therefore, the transition from the English subordinating conjunction to the Spanish subjectless verb appears to be a bit strange.

Consider next the sentence

- (14a) No creían en Jesús and then he sent este hombre...,

a combination of

- (14b) They didn't believe in Jesus and then he sent this man, and
 (14c) No creían en Jesus y entonces (luego) envió este hombre....

As in the preceding case, the English and Spanish sentences have nearly identical surface structures, with the exception of the optional subject pronouns in Spanish. Code-switching may therefore surmised to occur at several points in the sentence, and in fact other modifications such as

- (14d) No creían en Jesús and then he sent this man...and
 (14e) They didn't believe in Jesus y entonces envió este hombre...,

among others, were judged as acceptable code-switched examples. Another example is

- (15a) My Uncle Sam es el más agabachado,

a combination of

- (15b) My Uncle Sam is the most Americanized and

(15c) Mi tío Sam es el más agabachado.

The three sentences have identical superficial structures, and, while the shortness of the sentences precludes much variation, alternative code-switching possibilities, involving a shift immediately after the copula, were judged acceptable in both directions.

A further example, this time involving slightly less than perfect syntactic equivalence, is

(16a) He is doing the best he can pa no quedarsi atras,

a combination of (eliminating dialectal variants)

(16b) He is doing the best he can so as to not fall behind and

(16c) Hace todo lo que puede para no quedarse atras.

Although Spanish *quedarse* is not normally rendered by *fall* in English, but rather by such verbs as *be*, *remain*, *turn*, etc., it still fits in a more or less one-to-one syntactic congruence with the English verbs in question; thus, the sentences (16b) and (16c) are, for all practical purposes, syntactically equivalent, and code-switches at other points in the phrase are also tolerated by native speakers:

(16d) He is doing lo mejor que puede...etc.

(16e) Hace the best he can..., etc.

In all the above cases, a nearly perfect syntactic congruence exists between the Spanish and English equivalent sentences, thus supporting the strong form of Hypothesis A as regards the permissible environments for code-switching. In the majority of cases, however, such a perfect congruence does not exist; in certain instances, there is nearly total disparity in syntactic structure between parts of the semantically equivalent English and Spanish phrases. This is especially true in the case of clitic pronouns, which normally precede the verb in Spanish and follow it in English. The same problem also arises with many verbal locutions, where the equivalent of a simple English infinitive pattern is a more complex Spanish phrase involving use of the subjunctive mood. However, an overview of the cases of code-switching in which there is a lack of total syntactic equivalence between the English and Spanish sentences nonetheless suggests that congruence is still maintained in that portion of both phrases which falls after the actual switch. Thus, one may propose to modify Hypothesis A as follows:

Hypothesis B: Given the underlying semantic representation S , let X_1 and X_2 be the actual realizations of S in L_1 and L_2 respectively. Furthermore, for any point p_n in X_n (where $n = 1$ or 2), let a_n indicate that portion of X_n lying to the left of point p_n and b_n that portion of X_n lying to the right of p_n . In order

to produce a code-switched utterance by combining X_1 and X_2 with a break at p_1 and p_2 , it is necessary that b_1 and b_2 be syntactically equivalent.

Put in other words, the revised hypothesis states that, whereas the portion of a code-switched utterance that falls before the code-switch may indeed contain syntactically divergent elements, those portions falling after the switch must be essentially identical syntactically. The data from Spanish-English code-switching strongly support this revised hypothesis.

The sentence

(17a) No sé, porque I never used it,

is a combination of

(17b) No sé, porque nunca lo usé, and

(17c) I don't know, because I never used it.

With the exception of the placement of the clitic pronoun in Spanish, an automatic rule-governed phenomenon, the two sentences are essentially identical following the switch, although they are somewhat different in the initial portions. More examples are

(18a) A Sotero le gusta mucho cocinar barbecue every Sunday,

a combination of

(18b) A Sotero le gusta mucho cocinar barbecue cada domingo, and

(18c) Sotero likes to cook barbecue every Sunday.

The inherent syntactic difference in the first portion of the sentences, involving the contrast between Spanish gustar and English like, is a significant one that plagues students of the two languages for long periods of time. However, following the switch, the order of the constituents is identical. Attempts at placing the switch at virtually any other point in the sentence, where the elements following the switch would not be identical in the two languages, were rejected as anomalous by native speakers.

(19a) 'Tonces salió eso que she wanted to take mechanics,

a combination of

(19b) Entonces salió eso que (ella) quería seguir (coger)
mecánica

(19c) Then it turned out that she wanted to take mechanics.

Once again, the two sentences are virtually identical following the switch, although major syntactic differences do exist in the initial portions. The same holds for

(20a) I think I was mopping y me pegué asina,

a combination of

(20b) Creo que estaba limpiando el piso y me pegué así, and

(20c) I think I was mopping and I hit myself like this.

(21a) That has nothing to do con que le hagan esta

is combined from

(21b) Eso no tiene nada que ver con que le hagan esta, and

(21c) That doesn't have anything to do with them doing that to (him?).

In the first portion of the sentence, the idiomatic expressions have to do with and tener que ver con disallow a code-switch in their midst, while the final portion, consisting of regular, rule-governed verbal forms, although slightly different in superficial form, are regarded as sufficiently similar to permit a code-switch from one language to the other. Another example:

(22a) They only use English when they have to, like cuando van de compras;

(22b) Solo usan inglés cuando les hace falta, como cuando van de compras;

(22c) They only use English when they have to, like when they go shopping.

While there are several ways of rendering the initial part of the sentence in Spanish, all differ considerably from the common equivalents in English. Not so with the final portion, which is virtually identical in the two languages, thereby facilitating a code-switch at the indicated point.

Other useful examples of code-switching that conform to the pattern of Hypothesis B are:

(23a) Me duelen las manos porque las traigo tan quemadas from holding the steering wheel;

(23b) Me duelen las manos porque las traigo tan quemadas de (por) agarrar el timón (el volante);

(23c) My hands hurt because they're so scorched from holding the steering wheel.

The initial segments of the two sentences contain two radically differing expressions, marking one of the major syntactic differences between English and Spanish; the final portions, however, are nearly identical in each of the two languages. A code-switch at any point before the actual switch would be highly inappropriate and unacceptable because of the two differing syntactic constructions: the use of the intransitive verb doler (to

pain + indirect object) corresponding to English (to have something) hurt (someone) and the use of the verb traer (to carry) plus the definite article, lit. "I carry (have) them so scorched," where English would simply say my hands are scorched. Only after production of the sentence has passed the point where transfer to the other language would not entail a radical shift of syntactic structure does the actual code-switch occur.

An interesting example showing how a unified idiomatic expression may be considered as syntactically equivalent during code-switching despite radical differences in surface structure is found in the following:

(24a) And it's the first shag que se me hace que looks nice on a girl;

the Spanish equivalent would be

(24b) Y es el primer shag que se me hace que le va bien a una muchacha.

The idiomatic (Mexican) Spanish expression se me hace roughly corresponds to English it seems to me, and both require a following relative pronoun that/que. English looks good on, which is semantically an idiomatic expression (by Chafe's criteria) also corresponds to several slightly different Spanish expressions, the most common of which has been placed in (24b). A similar example would be:

(25a) But your complexion se ha compuesto mucho,

combined from

(25b) Pero tu complexión se ha compuesto mucho;

(25c) But your complexion has improved a lot.

While the portions of the sentences (25b) and (25c) following the switching point that yielded (25a) are syntactically different, due to the use of the reflexive verb in Spanish, the two verbs, componerse and improve/get better, are evidently felt to be semantically equivalent and it is this equivalence that permits the shift to another language. In both the English and Spanish versions, the portion of the sentences following the switch may be represented schematically as:

aux + verb + adv (intensifying);

the addition of the reflexive pronoun in Spanish is another instance of automatic, rule-governed insertion which in most cases does not interfere in any fashion with code-switching.

The preceding examples have offered only a rudimentary picture of the tremendous degree of complexity involved in the study of bilingual code-switching. A definitive examination of the

issues at hand will require a voluminous compilation of data, which would far exceed the bound of the present report. With the data currently available, however, it is possible to propose, in a highly tentative fashion to be sure, some conclusions regarding the cognitive structures which may underlie the sort of bilingual abilities that are conducive to code-switching. Ultimately, some sort of attempt must be made to integrate the linguistic and the extra-linguistic investigations of code-switching, to correlate the choice of purely linguistic environments which permit switches with the sociolinguistic and individual psychological parameters which, in effect, stimulate particular shifts. However, given the substantial lack of useful data on the latter issue, we must at present be content with an enumeration of certain necessary conditions for the occurrence of a code-switch, and leave for future investigation the determination of sufficient conditions. Indeed, since the role of individual idiosyncratic factors seems to be an important aspect of code-switching, in that among groups of approximately equal bilingual abilities, some code-switch more than others, a complete determination of the sufficient conditions for code-switching probably lies beyond the reach of the behavioral sciences. With regard to the linguistic constraints, however, the path toward an eventual model seems more clearly indicated. First of all, it is clear that, despite the superficial appearance of random and unprincipled behavior, bilingual code-switching does seem to obey a rather stringent set of sentential constraints. These constraints are of two fundamental types, intralinguistic constraints and interlinguistic constraints. The latter are more difficult to isolate, but they also offer greater interest to the psycholinguist. Perhaps the most general constraint, and the one most difficult to precisely specify, involves the actual quantity of code shifts that may be accommodated within a given stretch of discourse. Within a given linguistic community, there appears to be no single set of norms that determines how often, within a single sentence, languages may be shifted, nor how many words or syllables must intervene between switches. In general, more than one word normally occurs between switches, thus rendering less than totally acceptable combinations such as ESE or SES in three-word prepositional phrases, as noted earlier. Beyond such an almost trivial generalization, however, it becomes nearly impossible to offer more precise characteristics. Even careful observations of the speech patterns of a single person usually reveals no consistent behavior in terms of code-switching, although the most common tendency is for the sentence to begin in one language and, if it is reasonably short, to switch only once and end in the other language. In longer phrases, two or more switches are not uncommon, and, if one chooses to include examples of switched nouns, as noted above, the average number becomes even greater.

Of greater interest to the problem of bilingual competence is the general sort of syntactic constraint that appears to govern the global possibilities for code-switching. One such syn-

tactic constraint has been stated as Hypothesis B. While further investigation will be needed to refine and characterize the nature of bilingual code-switching, all available data on Spanish-English bilingualism appear to support at least a general version of Hypothesis B. It appears that, immediately preceding a code-switch, there occurs a brief moment of anticipation, in which, at some subliminal level, the basic syntactic structures of the remaining portions of the sentences in the two languages are compared and tested for congruence. The overall motivation behind such a comparison seems to be the achievement of a unified superficial syntactic pattern, regardless of the linguistic code in which the individual elements are represented. Therefore, code-switches which would in effect combine incompatible syntactic structures are normally avoided, and, when presented to native speakers in experimental situations, are routinely rejected as meaningless or absurd. Nonetheless, one may from time to time observe code-switches in which the post-switch portions of the sentences in the two languages exhibit less than perfect syntactic congruence. Again, it is too early to offer any definitive statement regarding the amount of congruence necessary to facilitate switching; this again may be a purely individual factor. The fact that perfect conformity is not normally required may be due to the nature of the anticipatory mechanism itself. It is in fact quite possible that this mechanism of anticipation, if it exists, does not proceed as a full-fledged syntactic analysis system, but rather as a sort of general pre-linguistic strategy, one which "chunks" the comparative phrases in the two languages for some overall measure of syntactic congruence but which is not fine enough to insure complete syntactic identity (cf. Cook 1969). That such may be the case does appear to be indicated by the failure of code-switches to consistently insure complete syntactic conformity; in those cases where radical departures from the dictum of syntactic congruence do in fact crop up, one is tempted to ascribe the deviations simply to personal idiosyncratic lapses, of the same sort that quite frequently occasion lack of grammatical concord, pronominal reference, etc. As such, code-switches of such a deviant nature could be regarded as production or performance errors and would not necessarily have a devastating effect on theories claiming the necessity for a higher degree of syntactic compability.

The data which have been surveyed suggest that the true model of bilingual competence may lie somewhere between the two diametrically opposite poles, being neither two completely distinct language systems nor one homogeneous amalgam. Those bilingual speakers capable of engaging in spontaneous code-switching apparently possess the ability to mentally compare equivalent sentences in the two languages for degree of syntactic compatability and to code-switch only in those instances where such compability would be preserved (cf. Clyne 1967, Rayfield 1970). Macnamara (1967: 71) speculates that "a language system can be made ready for a response even before the response has been de-

terminated." He also remarks (70) that "The most likely explanation [for code switching] is that [the bilingual] has the capacity to activate the L_2 system, carry out the semantic encoding, the selection of words and the syntactic organization while more or less mechanically producing in L_1 material which has already been prepared for production." Such a view is not contradicted by the present remarks, and yet Macnamara's view of code-switching as the combination of an active process of sentence generation and an automatic playback of a previously generated phrase does not really account for the high degree of syntactic conformity exhibited between L_1 and L_2 sentences following a code-switch. It is true that bilingual speakers seem to be able to manipulate the two language systems independently, and Macnamara's description quite adequately fits code-switching across sentence boundaries, that is, where one sentence is produced in L_1 and the following sentence in L_2 . In those cases where code-switching occurs intrasententially, however, it is impossible to speak of generating one sentence while mechanically producing another, since neither sentence is in fact produced in its entirety. A more plausible model would speak of the internal comparison, perhaps only in a very superficial fashion, of two simultaneously or rapidly-sequenced utterances, one in each language. The high degree of syntactic compatibility evident in cases of code-switching argues for a more nearly simultaneous comparison of variants, rather than a linearly-ordered production as suggested by Macnamara. If such simultaneous comparison does in fact occur in the cognitive structures underlying bilingual code-switching, this militates for the consideration of bilingual competence as more integral than two separate grammars. It is necessary to add some sort of additional stage, a comparator which juxtaposes the two systems simultaneously and allows for a change in the production code in mid-stream, as it were.

*This work is a continuation of an earlier paper presented to the Canadian Linguistic Association, Fredericton, N.B., June 1977, in which much of the background material, necessarily absent from this study due to space limitations, was presented.

References

- Clyde, M. 1967. Transference and Triggering. The Hague: Nijhoff.
- Clyde, M. 1969. Switching between language systems. Proceedings of the 10th International Congress of Linguists 1.343-9.
- Cook, V. 1969. The analogy between first and second language learning. IRAL 7.207-16.

- Espinosa, A. 1917. Speech mixture in New Mexico: the influence of the English language on New Mexican Spanish. in Stephens & N. Bolton, eds., The Pacific Ocean in History (New York: MacMillan), 408-28.
- Gumperz, J. 1964. Hindi-Punjabi code-switching in Delhi. PICL 9.115-24.
- Hasselmo, N. 1970. Code-switching and modes of speaking. In Texas Studies in Bilingualism, ed. Gilbert (Berlin: Walter de Gruyter), 179-210.
- Kolers, P. 1966. Reading and talking bilingually. Am. J. of Psych. 79.357-76.
- Lance, D. 1970. The codes of the English-Spanish bilingual. TESOL Quarterly 4.343-51.
- Lance, D. 1975. Spanish-English code-switching. In Hernandez-Chavez et. al., eds., El Lenguaje de los Chicanos (Arlington: Center for Applied Ling.), 138-53.
- Macnamara, J. 1967. The bilingual's linguistic performance: a psychological overview. J. of Social Issues 23.58-77.
- Redlinger, W. 1976. A description of transference and code-switching. In Keller et. al., eds., Bilingualism in the Bicentennial and Beyond (New York: Editorial Bilingue), 41-52.
- Selinker, L. 1969. Language transfer. General Linguistics 9.67-92.
- Selinker, L. 1972. Interlanguage. IRAL 10.209-31.
- Timm, L. 1975. Spanish-English code-switching: el porque y how-not-to. Romance Philology 28.473-82.
- Valdes Fallis, G. 1976. Social interaction and code-switching patterns: a case study of Spanish/English alternatives. Bilingualism in the Bicentennial and Beyond. pp. 53-85.

L'USAGE BILINGUE DANS LE CENTRE DE MONTREAL

Nicole Domingue*

McGill University

Lorsqu'on dit que Montréal est une ville bilingue, on exprime le fait que les deux langues officielles¹ y sont entendues couramment. L'anglais y a occupé jusqu'à maintenant une position dominante, bien qu'une majorité de 60% (ce chiffre est très approximatif) de la population soit francophone.² La politique linguistique de législation en faveur du français est encore trop récente pour avoir laissé des traces profondes dans l'usage. On verra plus loin, cependant, que certaines tendances semblent indiquer que l'usage bilingue est en train de changer.

La situation linguistique à Montréal est naturellement beaucoup plus complexe que ne le laissent entendre les lignes ci-dessus. Le bilinguisme mentionné est loin d'être uniforme: bien des quartiers de la ville pourraient être qualifiés d'unilingues. D'autre part, certains autres quartiers seraient plutôt trilingues ou multilingues. Les Montréalais prennent généralement pour acquis que l'anglais est la langue la plus souvent employée en public dans le centre-ville pendant la journée, les affaires étant traditionnellement liées à la population anglophone du Québec. Une étude préliminaire, faite en novembre 1976, avait montré qu'en fait il n'en est pas ainsi: l'usage du français l'emporte quelque peu sur celui de l'anglais. L'enquête, dont les conclusions sont rapportées ici, avait pour but de vérifier cette situation et peut-être de lui donner une explication.

Pour ce faire, une étude de moyenne envergure a été entreprise par un groupe d'étudiants de l'Université McGill afin d'observer le comportement linguistique de sujets qui fréquentent le centre-ville pendant la journée. Six équipes de deux chercheurs chacune se sont postées à proximité de kiosques de renseignements dans certains endroits publics (centres d'achats, magasins, stations de métro)³ et ont observé, durant des périodes de deux heures au cours de quatre après-midi successifs, l'usage linguistique de 456 personnes. Un des membres de l'équipe était chargé de noter la langue du sujet, au moment où il (ou elle) s'approchait du kiosque pour demander un renseignement; le sexe et l'âge approximatif du sujet étaient également relevés. Quand ce dernier s'en allait, l'autre membre de l'équipe l'arrêtait pour lui demander quelle

*Cette étude est en fait le produit du travail collectif de l'auteur et de ses étudiants: Samia Bazerghi, Jeanne Borden, Cherine Chehata, Aniello Covone, Susan Desjardins, Lyn Doole, Lina Perrone, Lars Pira, Robin Pitsiladis, Meryl Siegal et Christiane St-Aubin.

langue il utilisait chez lui. Le tableau I montre la composition démographique de l'échantillon de locuteurs obtenu. La population

TABLEAU I
Composition par sexe et par âge

Age	F	M	Total
0-20	34	16	50
21-35	93	89	182
36-50	77	60	137
51-	56	31	87
Total	260	196	456

féminine est plus nombreuse que la masculine et les groupes d'âge moyen sont les mieux représentés. Comme on le verra plus loin, ces disparités devront être prises en considération lors de l'évaluation des résultats.

L'analyse.

Comme il était prévu, l'enquête montre que, parmi les sujets de l'échantillon, l'usage est plutôt favorable au français: 52% utilisent cette langue, alors que 48% se servent de l'anglais.

La première remarque qui s'impose est que cette différence reflète simplement le fait que plus de francophones que d'anglophones se trouvaient en ville au moment de l'enquête. Le tableau II montre que les francophones sont en effet un peu plus nombreux que les anglophones, mais pas suffisamment pour expliquer l'écart observé. L'utilisation plus importante du français en

TABLEAU II
Langue parlée à la maison

français	44% (200) ⁴
anglais	42% (191)
autre	14% (63)

ville doit donc être due, en partie, à la décision de certains non-francophones de s'exprimer en français en public. Le tableau III montre comment les groupes d'origines linguistiques différentes se comportent vis-à-vis de l'usage du français.

TABLEAU III
Langues parlées à la maison et utilisées en ville

		Langue parlée à la maison		
		français	anglais	autre
langue utilisée en ville	français	94% (187)	16% (30)	34% (22)
	anglais	6% (13)	84% (162)	66% (42)

Le choix linguistique de ceux qui ne parlent ni français ni anglais à la maison n'est pas favorable au français et confirme l'axiome bien connu que les immigrants ont tendance à adopter l'anglais, langue traditionnellement rattachée au désir de mobilité sociale et à l'ouverture du marché du travail. Ce ne sont donc pas les immigrants qui contribuent à la prédominance du français, si ténue soit elle.

Il faut donc chercher ailleurs et remarquer qu'une proportion de 16% d'anglophones changent de langues dans la situation observée. Ceci va à l'encontre de l'opinion générale qui voudrait que ce soit les francophones qui permutent. Il est probable, si on en croit les Montréalais de vieille souche, que cette tendance est très récente, si récente qu'elle n'est d'habitude pas reconnue. Cette attitude nouvelle envers le français peut être le résultat de la législation sur la place de cette langue dans la vie publique ou celui d'un climat plus favorable à son utilisation parmi les Montréalais, tant anglophones que francophones. Il est à remarquer que peu de francophones permutent (6%).

Une analyse de l'usage linguistique par groupes d'âge montrerait peut-être que ces nouvelles attitudes sont reflétées par les générations les plus jeunes. Il faut, en conséquence, se tourner vers les données par groupes d'âge.

Les groupes d'âge.

Si on examine le tableau IV, on peut facilement voir que l'usage de chacune des deux langues officielles n'est pas constant et varie suivant l'âge des individus. Les personnes des deux groupes d'âge moyen utilisent le français plus que l'anglais.

TABLEAU IV
Usage des langues par groupes d'âge

Age	Usage du français	Usage de l'anglais
0-20	42% (21)	58% (29)
21-35	55% (100)	45% (82)
36-50	58% (79)	42% (58)
51-	45% (39)	55% (48)

Au contraire, 42% seulement des moins de vingt ans utilisent le français et 45% des plus de cinquante ans. La plus importante utilisation de l'anglais par les plus de cinquante ans exprime peut-être le fait que ces personnes sont toujours fidèles à la coutume qui voulait que l'anglais soit la langue publique dans le centre-ville.

L'usage des moins de vingt ans, par contre, ne corrobore pas l'hypothèse que les jeunes générations reflètent plus directement la tendance vers une plus grande utilisation du français. Le tableau V explique ce phénomène: les sujets francophones de moins de vingt ans sont moins nombreux, dans notre échantillon, que les sujets anglophones, alors que, pour les groupes d'âge moyen, le

nombre des francophones l'emporte sur celui des anglophones. Les

TABLEAU V

<u>Langue parlée à la maison et groupes d'âge</u>			
Age	français	anglais	autre
0-20	17	25	8
21-35	84	68	30
36-50	63	60	14
51-	36	39	12
Total	200	192	64

sujets de plus de 51 ans sont, eux, plus souvent anglophones. Le tableau VI, qui considère seulement les personnes de langues française ou anglaise, fait apparaître cette situation plus distinctement. La disparité est aussi plus marquée pour le groupe de 0 à 20 ans, ce qui confirme d'autant mieux les données du tableau IV.

TABLEAU VI

<u>Groupes d'âge par langue parlée à la maison</u> <u>(français et anglais seulement)</u>		
Age	français	anglais
0-20	40%	60%
21-35	56%	44%
36-50	51%	49%
51-	48%	52%

Cette conclusion est renforcée par l'observation qu'aucun des francophones de ce groupe n'a utilisé l'anglais au cours de l'enquête. La prédominance de l'usage de l'anglais dans les groupes d'âge périphériques est, par conséquent, due à la composition de ces groupes.

Quant à l'usage prédominant du français, il est dû à deux causes: la composition générale de l'échantillon qui est un peu plus francophone qu'anglophone (51% contre 49%) et le comportement linguistique des sujets anglophones qui, si on se reporte au tableau III, montrent une certaine tendance à permuter. Ceci indiquerait qu'un changement, si imperceptible soit-il, est en train de prendre place dans l'usage linguistique montréalais.

Le sexe.

La dimension du sexe doit être aussi prise en considération, si le comportement des Montréalais doit être expliqué d'une manière pertinente. Sur le tableau VII, l'usage des deux sexes paraît être parallèle, mais les différences de comportement entre les groupes d'âge sont plus marquées dans la population féminine. Peut-être

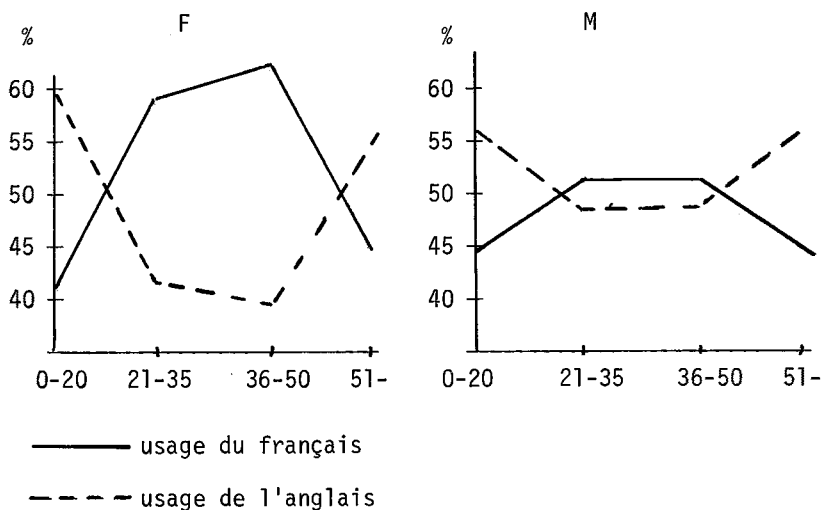
TABLEAU VII

Usage bilingue par âge et par sexe

Age	usage du français		usage de l'anglais	
	F	M	F	M
0-20	41%	44%	59%	56%
21-35	58%	52%	42%	48%
36-50	62%	52%	38%	48%
51-	45%	45%	55%	55%

que le plus grand nombre de personnes de sexe féminin interrogées (57% de l'échantillon est composé de femmes) produit cette différence entre les sexes, mais ceci n'est guère probable puisque l'échantillon masculin est loin d'être insignifiant (196 personnes). La figure 1) fait ressortir la disparité entre les usages masculin et féminin. Celle-ci est particulièrement marquée dans les groupes d'âge les mieux représentés, c'est-à-dire de 21 à 50 ans.

Figure 1)



Il faudrait donc voir quelle est la composition de ces groupes d'âge par sexe et par langue parlée à la maison. Si ces deux

TABLEAU VIII

Langue parlée à la maison par sexe

Sexe	français	anglais
F	58%	42%
M	49%	51%

groupes, qui semblent les plus significatifs, sont combinés, la solution au problème de la disparité entre le comportement des hommes et des femmes apparaît: chez les femmes, le pourcentage des francophones l'emporte sur celui des anglophones, alors que la situation est inverse pour les hommes. La prédominance du français à ce niveau d'âge reflète simplement le fait que les femmes francophones constituent le groupe le plus important des personnes qui se trouvent en ville l'après-midi. La présence de facilités d'achat explique assez bien cette situation.

Un autre facteur entre peut-être en jeu pour expliquer le comportement des sujets masculins. Il semble que ces derniers soient un peu plus portés à changer de langue en public que les sujets féminins: 17% des hommes anglophones contre 15% des femmes permutent vers le français, et 7% des hommes francophones contre 6% des femmes utilisent l'anglais. Ceci doit contribuer quelque peu à la régularisation de l'usage masculin. De plus, parmi ceux qui ne parlent ni français ni anglais à la maison, les hommes montrent un écart moins grand que les femmes dans leur choix entre les deux langues publiques. Ceci aussi doit contribuer à réduire les disparités entre les groupes d'âge masculins (voir tableau IX).

TABLEAU IX

Langue d'usage des immigrants par sexe

Sexe	usage du français	usage de l'anglais
F	30%	70%
M	39%	61%

Il est difficile de conclure de ces chiffres que les immigrants masculins montrent une préférence pour l'anglais moindre que celle des immigrantes, car, sur les soixante-trois personnes interrogées qui ne parlent ni le français ni l'anglais à la maison, des locuteurs de pas moins de quatorze langues ont été retenus. Certains viennent de pays où le français ou l'anglais sont des langues secondes largement employées (par exemple le français en Afrique du Nord et l'anglais en Inde). L'échantillon obtenu est trop minime pour décider de l'usage de ceux qui ne tombent pas dans cette catégorie.

En conclusion, le sexe semble avoir une influence légère sur le comportement linguistique, qui est le résultat ou d'une meilleure formation linguistique chez les hommes ou d'une certaine facilité à se plier à un nouvel usage.⁵

Quelques remarques supplémentaires.

La majorité des sujets semblait intéressée par cette enquête et de nombreuses personnes ont donné aux enquêteurs plus de renseignements sur leur comportement linguistique qu'on ne leur demandait. Ces renseignements n'ont pu être utilisés pour l'étude principale, étant trop disparates, mais il a semblé intéressant de rapporter quelques-unes des impressions générales des enquê-

teurs. Les anglophones unilingues ont semblé s'inquiéter beaucoup plus que les francophones de la raison et du but de cette enquête. Certains locuteurs ont défendu leur absence de bilinguisme par des remarques agacées ("Je suis canadienne-française!!" ou "So, I only speak English..!!"); d'autres se sont excusés de leur manque de connaissance du français ("Well, dear, I tried to learn that French, but it's difficult, you know").

Plus de femmes que d'hommes ont déclaré être bilingues sans qu'on leur pose la question. Au contraire, les hommes dont l'usage bilingue était observé ne faisaient pas de commentaires sur leur capacité. En général, les hommes ont donné aux enquêteurs l'impression d'être plus sûrs d'eux-mêmes que les femmes: ils ne tentaient guère d'expliquer leur usage.

Ces remarques, qui concernent les attitudes des sujets parlants vis-à-vis des langues et peut-être de la législation sur l'usage linguistique, devraient suggérer la nécessité de recherches systématiques sur les attitudes.

Conclusion critique

Une enquête qui cherche à déterminer quelque chose d'aussi vaste que l'usage de langues ne peut être valide que si elle est conduite d'après des techniques rigoureuses. Dans le cas de cette étude, les endroits publics choisis l'ont été, plus parce qu'ils étaient pratiques ou accessibles, que par hasard. D'autre part, il est probable que les personnes qui se déplacent dans le centre-ville pendant l'après-midi ne sont pas représentatives de la majorité de la population qu'on peut trouver pendant toute la journée; à d'autres heures, les personnes qui travaillent dans les bureaux et celles qui se déplacent pour affaires seraient plus visibles (particulièrement entre midi et une heure). Il faut aussi tenir compte du fait que beaucoup de gens vont en ville en voiture, c'est-à-dire que les échantillons pris dans les stations de métro et la gare sont moins représentatifs que les autres.

D'autres facteurs peuvent aussi avoir biaisé les résultats: les échantillons de chaque sexe, de chaque groupe d'âge, et des groupes par langue parlée à la maison devraient être de taille comparable. Certains sont trop restreints pour donner des résultats valides, particulièrement le groupe d'âge 0-20. Quant au groupe de sujets ne parlant ni français, ni anglais, à la maison, il a dû être traité en un bloc, les différentes origines des sujets étant trop variées pour leur petit nombre dans l'échantillon.

Les conditions mêmes de l'enquête auront peut-être produit des réponses peu consistantes de la part des sujets. Par exemple, les équipes ont quelquefois varié la manière de poser la question: "Quelle langue parlez-vous à la maison?" ou "Parlez-vous français/anglais à la maison?". Il est aussi possible que les investigateurs, tous bilingues mais non sans accent, aient influencé les réponses: les sujets ont quelquefois l'impression qu'ils doivent donner une réponse préférée par l'enquêteur. De plus, dans les environnements bruyants qui étaient nécessaires à cette enquête, les mots "à la maison" n'ont peut-être pas toujours été bien entendus, et les réponses données peuvent avoir eu tendance à reproduire la langue qui venait d'être utilisée. Enfin, la question,

telle qu'elle était posée, ne pouvait obtenir qu'une réponse; dans les cas où il y a multilinguisme à la maison, le sujet devait nécessairement faire un choix. Il est impossible de savoir si ce choix s'est fait en faveur de la langue maternelle ou en faveur de la langue parlée le plus souvent. Il a semblé aux investigateurs que cette difficulté n'altérerait pas vraiment les sens de la recherche puisque leur but était de découvrir si oui ou non les locuteurs se sentent obligés de changer de langue dans une situation bien déterminée. La réponse des locuteurs, si elle indique une permutation, indique simplement l'impression du sujet qu'il fait un effort spécial ou qu'il n'en fait pas lorsqu'il se trouve dans une situation publique en ville.

Compte tenu des observations qui viennent d'être faites, on peut malgré tout dire que cette enquête a montré trois faits intéressants: le premier est que le centre-ville de Montréal n'est pas un bastion de l'anglophonie, le deuxième que les anglophones, plus que les francophones, se sentent obligés de permuer, et le troisième, que la majorité des sujets de langue française ou anglaise s'attendent à être servis dans leur langue.

NOTES

1. Le français et l'anglais sont les deux langues officielles du Canada. La Loi sur les Langues officielles (Loi 22), adoptée au Québec en 1974, a fait du français la langue prioritaire. Le Projet de Loi I veut faire du français la seule langue officielle du Québec.
2. Au Québec, on peut généralement considérer comme "francophones" ceux dont la langue maternelle est le français et qui parlent français à la maison, et "anglophones" ceux dont la langue maternelle est l'anglais et qui parlent anglais à la maison.
3. L'enquête a été faite aux kiosques de renseignements des magasins Eaton's et La Baie, des centres d'achats de la Place-Ville-Marie et de la Place Bonaventure, de la station de métro Peel et de la gare du C.N.
4. Les chiffres entre parenthèses indiquent les nombres absolus.
5. On pourrait se demander si ce comportement est une expression de "prestige inverse" attribué au français par certains locuteurs. Cette notion, que P. Trudgill appelle "covert prestige", veut que certains sujets parlants, surtout masculins, soient attirés par des niveaux de langue non caractéristiques des classes sociales privilégiées.

ATTITUDES TOWARD VARIETIES OF SPANISH¹

Jon Amastae - Pan American University
Lucía Elías-Olivares - U. of Illinois, Chicago Circle

O. There have been several significant studies of attitudes toward the "accented" English spoken by Mexican-Americans, in addition to many informal references in the literature to accented English and attitudes toward it (cf. Barker 1974 for example). Galván, Pierce and Underwood 1976a have shown that prospective teachers rate Mexican-American "accented" English lower than non-Mexican-American accented English; and Galván, Pierce, and Underwood 1976b report (disturbingly) that university training in linguistics does nothing to alter these attitudes, which are apparently very deeply rooted. Similar results are reported by Arthur, Farrar, and Bradford 1974. Thompson 1973 notes that often Mexican-Americans may adopt a monolingual Texas regional style of English speech because they feel that such a speech style will increase their career opportunities.² Ryan and Carranza 1975 confirmed the prediction that the functional separation of language varieties would be reflected in evaluative reactions toward speakers of standard and accented English. All groups tested rated the accented speakers higher in a home context than a school context. Ryan and Carranza's (1975) hypothesis that Mexican-Americans would prefer Mexican-American accented speakers in the home context with respect to solidarity scales was not confirmed, however.

Much less research concerning attitudes toward varieties of Spanish exists. Ornstein and Goodman 1974, Ornstein, MacIntosh 1974 notes that in the bilingual setting of El Paso a significant number of students and teachers classify the local Spanish as "border slang"; Elías-Olivares 1977 notes a similar phenomenon in the Lower Rio Grande Valley of Texas. Elías-Olivares 1976 notes a common feeling of linguistic inferiority regarding Spanish among the older informants, while younger informants often regard their ways of speaking as expression of ethnic pride. She also notes that speakers may "code-switch" in order to demonstrate their bilingualism and their lack of assimilation into the larger anglo culture. Gumperz and Hernández-Chávez 1972 report similar findings concerning code-switching. Hannum 1975 points out that Spanish has an essential role in a bilingual's feeling of individual and familial well-being, but that it is not as important in other domains. For an excellent detailed discussion of most of the studies we have mentioned (and more) see Ryan and Carranza 1976.

This paper is concerned with attitudes toward the various types of Spanish found in the Lower Rio Grande Valley of Texas (and in other Spanish-speaking areas of the Southwest). We use both the semantic differential scales developed by Osgood and Suci 1955 and data taken from interviews with informants. The latter constitutes an ethnographic approach to the study of language use and it proves a valuable second perspective to the semantic differential method of studying linguistic attitudes.

1. Method. We played a tape containing five samples of Spanish to various populations of respondents. The tape included standard Spanish, popular Spanish, Spanish with loan translations, code-switching, and *caló*. (For discussion of these varieties, see Elías-Olivares 1976a.) These passages differ lexically, syntactically, and phonologically, and were taken from actual conversation in interviews for other studies.³

Many attitude measurements have used the same passage, with the variable being only phonological, that is, presence or absence of "accent" of one type or another. However, Agheyisi and Fishman 1970 have criticized this practice on the grounds that evaluators may react to incongruities between the content and the style of the passages. Therefore we have used passages which differ completely. Since some of the varieties of Spanish are radically different from the others in domain of usage, it would have been completely false to attempt to use one passage (in terms of lexicon and syntax) for all varieties.

We have studied four primary populations: Pan American University students (bilingual (N=27) and monolingual (N=12)) who have had a linguistics course (Total N=39); Pan American students who have not had a linguistics course (bilingual (N=16) English monolingual (N=14); Total (N=30)); Pan American University foreign students (Spanish speakers from Mexico and other Latin American countries) (N=16); and Mexicans who reside in the border zone (N=40). Part of the experiment materials included a background information page. Thus, we have also studied various sub-populations, selected by age, education, birthplace, place of schooling, etc.

Each respondent was given a booklet containing the background sheet and the rating sheets. The rating sheets contained fourteen semantic differential scales consisting of bi-polar adjectives. These scales were, of course, randomized so as to eliminate any obvious pattern between positive and negative poles. The scales were designed to reflect the dimensions of affective meaning found by previous investigators to be significant: evaluation, potency, activity, and solidarity. Five of the scales - nice/mean, polite/impolite, beautiful/ugly, understandable/not understandable, and good/bad - were a priori designed as solidarity stressing. Nine of the scales - good/bad, smooth/rough, smart/

stupid, even/un-even, beautiful/ugly, patient/impatient, understandable/not understandable, polite/impolite, careful/sloppy - were a priori designated as evaluative in an overall sense. Six of the scales were a priori designated as potency-activity scales - strong/weak, sharp/dull, careful/sloppy, hard/soft, fast/slow, smooth/rough. The a priori groupings were established with reference to previous literature in the area, and were also tested by factor analysis. All of the materials were in Spanish for those groups which were predominantly Spanish-speaking. Each scale contained six spaces between each polar adjectives; respondents marked the space which seemed to them to typify the passage, e.g.

fast _____ X _____ slow

if the passage seemed slightly slow.

1.1 As mentioned earlier, there were two versions of the experiment booklet, one in English and one in Spanish. The experimental procedure for the Pan American University students was conducted in English; for the PAU foreign students and both the Mexican groups the procedure was in Spanish. The respondents were told that the experiment was designed to test reactions to intonation contours only and that they were to consider only intonation contours, not lexical, syntactical, or other aspects of each passage. Each passage was played twice, and the recorder turned off while respondents marked the rating sheets. The experimenters paid close attention to the respondents during the procedure, observing and noting reactions, comments, etc.

1.2 We initially hypothesized that:

1) Mexicans and PAU foreign students would rate all varieties except for standard Spanish negatively on all scales, and that for the foreign students and residents of the border zone this negative reaction would be severe.

2) Mexican-Americans would rate all but the standard Spanish negatively, except on the solidarity scale, where the ratings would either be positive or significantly less negative. This pattern might be reversed for the standard Spanish.

3) There would be significant correlations between birthplace, place of residence, education, age, and linguistic attitudes.

In another study of this university, when asked if the Spanish of the area was different than the Spanish spoken in Mexico, 90% said that people in Mexico spoke "correct Spanish" while people in the Rio Grande Valley spoke bad Spanish, pocho, mocho, not "real" Spanish. Thus, these hypotheses were established on the assumption that the varieties of Spanish would function for Mexi-

can-Americans in a manner parallel to the manner in which varieties of English function (cf. Ryan and Carranza 1975), and upon the desire to test the often heard remark that Mexicans and other Latin-Americans deprecate the Spanish of Mexican-Americans.

2.0 Analysis. The data of the ratings were first subjected to factor analysis to test the initial a priori groupings of scales into factors. Factor analysis was applied to the total matrices for the fourteen adjectives. These matrices were computed with the total ratings given by the evaluators for the five samples. The resulting 14 X 14 correlation matrix was subjected to a principal factor analysis using an eigen value of .9 as a criterion for extraction. The extracted factors were then rotated by the varimax method.

Table 1 gives the factor loadings for the adjective scales on the four extracted factors. These factors accounted for 68.8 percent of the variance in the data. Factor one was identified as the evaluative dimension, factor two as the activity dimension, factor three as the solidarity dimension, and factor four as the potency dimension.

The factor analysis was calculated for each principal population separately than for the total sample. This was done because we suspected that the evaluative and solidarity dimensions would show the clearest separation for the Mexican-Americans. This, indeed, was the case. However the same calculations for the total sample are not materially different. (The factor loadings given above, Table 1, are from the total sample). In summary here, the factor analysis demonstrated the accuracy of the a priori groupings of scales.

With the factors identified by factor analysis as well as a priori, we collapsed the activity/potency factor. We then used the three resulting factors, evaluation, solidarity, and activity/potency - as the dependent variable in the ANOVA with the other variables (main group, native language, age, sex, etc.). We also computed an overall rating for each variety consisting of a mean of all the adjectives for each sample.

3.0 Discussion. Figure 1 shows overall mean ratings on the six-point scale for population X language variety. As hypothesized there is a significant effect between main population and overall rating of popular Spanish, mixed Spanish, and code-switching ($F=13.699$ $p<.002$, $F=18.484$ $p<.001$, and $F=5.997$ $p<.001$ respectively), the Mexicans and foreign students rating them significantly lower (cf. fig. 1). On the other hand, all populations rated caló and standard Spanish much the same, the former almost uniformly negatively and the latter almost uniformly positively. However it is interesting to note that even the Standard Spanish is rated only slightly positively by the Mexicans and the foreign students. This may be due to the fact that these two groups have

such well-developed prejudices against Mexican-Americans and their speech habits that the fact that the Standard Spanish was included with varieties often identified with Mexican-Americans was enough to induce them to down-grade the Standard Spanish sample. We had not expected that the PAU foreign students, the group which has the most contact with the varieties of Mexican-American Spanish spoken on the PAU campus, would show significantly lower ratings of all varieties. We have no explanation for this.

These negative reactions to Mexican-American varieties of Spanish are also illustrated by the behavior and comments of the foreign students and Mexican evaluators during the session. At every sample (except for the Standard Spanish) there was a great deal of laughter and mimicry. During the caló sample one fellow in the Mexican group turned to a neighbor to joke using a more exaggerated version of the intonation in the sample, "eh, vato, ¿cuándo te grabaron?" During the session with the foreign students, one of the experimenters had a conversation with one of the student which went something like this:

Experimenter: ¿Qué piensan de los ejemplos?

Student:1 El tercero estaba mas o menos pero los otros estaban de la patada.⁴

Experimenter: ¿Por qué? ¿Pueden darme unos ejemplos?

Student:2 No se dice "trujo", la palabra es "estrujo".⁵

Experimenter: Pero hay mucha gente en Latinoamerica que dice "trujo".

(They both look surprised)

Student:1 Si, pero es la gente del campo, rural. No saben hablar.

Student:2 ¡Es mal español!

Experimenter: Bueno, solamente es diferente.

(laughter)

Student:1 No, es malo, no es correcto.

Student:2 Es español pocho, de aquí.

In short, it appears well substantiated that Mexicans and other Latin Americans do have clear negative reactions to Mexican-American Spanish, extending even to one variety, popular Spanish, which clearly is not greatly different from popular Spanish spoken in Mexico and other Latin American countries. The negative reactions to this and the other varieties appear to be caused by reactions to Mexican-Americans themselves.

Examinations of the mean ratings of Mexican-Americans also shows interesting results. (Refer again to Figure 1). The Standard Spanish was rated highest, higher than by the foreign students and Mexicans. The popular Spanish and mixed Spanish were also given positive ratings by both groups of PAU students, though they were not as highly rated as the Standard Spanish. An explanation for the fact that the mixed Spanish is rated higher than the popular Spanish is apparently provided by D'Anglejan and Tucker

(1973). They hypothesized that speakers are more aware of phonological than syntactic variables. Since in our tape the mixed Spanish was produced with an intonation contour more like Standard Spanish, the popular Spanish would naturally be regarded as the more stigmatized of the two. Only the caló and the code-switching were evaluated negatively, with the caló receiving the lowest rating. We find several interesting aspects of these results. Three of the five varieties are evaluated positively by Mexican-Americans, including two which are often said to be stigmatized varieties, (the popular Spanish and the mixed Spanish). This appears to substantiate Elías-Olivares' (1976) findings that younger Mexican-Americans are beginning to regard their language with ethnic pride, despite certain negative attitudes about it. It may also be that these never were stigmatized varieties for these speakers. Linguists who asserted that they were may have been in error, or informants who claimed they were stigmatized may have been responding to what they thought an investigator wanted to hear.

These figures also show clear difference in evaluations between the group of PAU students who have had a class in linguistics and those who have not ($F=13.699$ $p<.002$). Those who have had linguistics give significantly higher ratings than those who have not had linguistics. Though the caló and code-switching are still rated negatively by the linguistics students, these negative ratings are significantly less negative than those given by the non-linguistics students, and in the case of code-switching, the rating (3.405) is very near the neutral point. This result appears to contradict directly the findings of Galván, Pierce, and Underwood 1976b that linguistic study has no effect in erasing deeply rooted linguistic prejudices. However, a crucial difference between the studies may be the fact that Galván, Pierce, and Underwood were dealing with outgroup reactions (Ryan and Carranza 1976) while we are here concerned with ingroup reactions. That is, it may be that linguistic study does relatively little to alter prejudicial reaction to the speech of others, while it does more to alter one's self-estimation. Another factor might be differing content and/or emphasis in the courses taken by the groups being compared. This needs, of course, further study.

In the session with the PAU students, there was little of the derision and mimicry which characterized the session with the foreign students and Mexicans, although the caló and the code-switching did elicit a few giggles. One student was heard to ask, "We're just supposed to consider intonation? Even if they don't pronounce the words right, or use the right words?", while another said, "Well, that's the way we talk most of the time."

Up to now we have discussed only overall ratings, computed as mean ratings for the fourteen adjective scales. Figure 2 shows ratings on the evaluative scales.

The foreign students and Mexicans give significantly lower ratings on all scales to most varieties. The PAU linguistics students show significantly higher ratings on all scale types for most varieties.

The evaluation and solidarity ratings for popular Spanish are significantly higher for PAU students (both linguistics and non-linguistics) than for foreign students and Mexicans ($F=16.28$ $p<.001$), ($F=14.133$ $p<.001$ respectively). The evaluation ratings of mixed Spanish are significantly higher for PAU students than for foreign students and Mexicans. In addition the solidarity ratings of mixed Spanish are significantly higher for PAU students than for foreign students and Mexicans ($F=9.408$ $p<.01$). While there are no significant effects involving population X evaluative or solidarity dimension for caló, the activity/potency factor is important for caló ($F=14.493$ $p<.001$). Finally, there are important differences in the evaluative ($F=7.114$ $p<.001$), activity/potency ($F=15.146$ $p<.001$), and solidarity ($F=7.605$ $p<.001$) dimensions of the ratings of code-switching by PAU students and by the foreign students and others. These results show that popular Spanish, mixed Spanish, and code-switching are rated higher on the solidarity dimension than on the evaluative dimension by PAU students (Figure 3).

In addition Sex and Age are important variables for some varieties. Age X evaluation of mixed Spanish shows a significant effect ($F=3.696$ $p<.05$), as does age X solidarity of mixed Spanish ($F = 3.402$ $p < .05$). Finally, sex is an important factor in the evaluative dimension ($F=4.872$ $p<.05$) and solidarity dimensions ($F=5.296$ $p<.01$) of mixed Spanish.

Finally, the data reveal some other interesting patterns. Table 2 shows mean overall ratings of the five varieties by a sub-set of the PAU student population: native Spanish speakers and non-Spanish speakers. Even though the samples were all in Spanish, we included non-Spanish speakers in the study to see if they, while not understanding Spanish, nevertheless identified and stereotyped varieties commonly heard. Table 2 shows that while the ratings given by non-Spanish speakers are lower than those given by Spanish speakers, they are not significantly lower, and indeed, show the same hierarchy from positive to negative. Thus, the reactions of non-Spanish speakers are much the same as those of Spanish speakers to Spanish varieties commonly heard in the PAU speech community. It very much appears that, even though non-Spanish speakers do not understand Spanish, they have built up clear social stereotypes and attitudes which are associated with and called by the hearing of a non-understood (in the usual sense) language.

In addition, some differences in the reactions between males and females are interesting though not statistically significant.

Table 3 shows mean overall ratings of the five varieties by males and females. Here, males seem to rate the varieties more extremely, with the caló and code-switching being rated more negatively by males, and Standard Spanish, and popular Spanish rated more positively by males. Much the same pattern is revealed in Table 4, which breaks the evaluations down into dimensions.

Here also the males tend to rate the caló (except on the activity/potency scale) and the code-switching lower than the females. Males also rate the Standard Spanish and popular Spanish higher than the females. The higher male rating of the caló on the activity/potency scale is not surprising, but the other lower ratings are a bit puzzling. In view of the fact the caló is usually considered within the community as a masculine style, one would expect men to rate it higher than women would on all counts. Moreover, (and conversely) it is not surprising that men consider Standard Spanish lower on the activity/potency scale, but we are slightly surprised by the men's higher ratings of Standard Spanish on the evaluative and solidarity scales. This is definitely an area for further investigation.

Of our original hypotheses, the first, that Mexicans and other Latin Americans tend to deprecate most varieties of Mexican-American Spanish, was clearly confirmed. The second, that Mexican Americans tend to deprecate their own language, was partly confirmed, partly disconfirmed. The third, that there would be other significant factors, was also shown. Additionally, we have shown other interesting dynamics of the linguistic attitudes of this speech community, in particular the fact that the solidarity dimension is an important part of linguistic attitudes, that the evaluative dimension and the solidarity dimension do not necessarily run exactly parallel, and that hearers categorize others on the basis of the sound of language varieties heard frequently but not necessarily understood.

4.0 Further Considerations. Aside from the direct results concerning the dynamics of Spanish varieties in this speech community, these findings have other implications. It is extremely too important to realize that all of the varieties were recognized as common varieties in the community by the Mexican-American evaluators. Thus it is not possible to speak blithely of "Chicano Spanish" as though it were a monolithic entity. The fact there are not only several varieties of Spanish in the community but also a well-developed complex of attitudes toward these varieties should be considered in the designing of educational programs.

FOOTNOTES

¹The activity which is the subject of this report was supported in whole or in part by the U. S. Office of Education, Department of Health, Education and Welfare. However, the opinions expressed herein do not necessarily reflect the position or policy of the U. S. Office of Education, and no official endorsement by the U. S. Office of Education should be inferred. The research reported here has been supported by an Advanced Institutional Development Grant OEG-74-2511 to Pan American University, which support is gratefully acknowledged.

We would also like to thank all those who participated in the study, as well as those who have given helpful comments and assistance on various aspects of this work, particularly Bob Wrinkle and Jorge Herrera. All errors are, of course, entirely our own.

²This is not supported by Elías-Olivares 1977, however.

³Cf. Lambert, Hodgson, Gardner and Fillenbaum 1960, D'Anglejan and Tucker 1973. Anifeld, Bogo, and Lambert 1962; Tucker and Lambert 1969, Williams, Whitehead and Miller 1975; and Carranza and Ryan 1975 for other uses of these techniques.

⁴de la patada, "really bad", < 'kick' "

⁵trujo - estrujo: trujo is a well known archaic form of the preterite of traer (std. trajo) occurring throughout the southwest. Here the student simply misheard the word as being a form of estrujar, "to wring."

	Factor 1	Factor 2	Factor 3	Factor 4
Fast	-.03614	-.20767	-.97200	.04274
Strong	.20210	-.01825	.07153	.76035
Smooth	.67625	.36820	-.02730	-.07343
Understandable	.39840	.05550	.21315	.13800
Even	-.03885	.65220	-.02083	.33523
Good	.77710	.13461	.33002	.09265
Beautiful	.81059	.11137	.15914	.09553
Careful	.81021	.13065	.23377	.02048
Smart	.77487	.13043	.22719	.06798
Hard	-.38960	-.39933	-.40744	.19610
Patient	.32429	.40394	.20521	-.12767
Polite	.67178	.15398	.37872	.12286
Nice	.39044	.28547	.79245	.15997
Sharp	.78903	-.01909	.07755	.11202
Cumulative Percent Of Variance	43.8	53.3	61.9	68.8

FACTOR LOADINGS FROM THE ROTATED FACTOR MATRIX
FOR THE SEMANTIC DIFFERENTIAL RATINGS

TABLE 1

ALL PAU STUDENTS (NON-FOREIGN)		
	Spanish Speakers	Non-Spanish Speakers
Pop Spanish	3.945	3.714
Mixed Spanish	4.336	4.005
Std. Spanish	4.469	4.184
Caló	2.882	2.798
Code-Switching	3.228	3.069

Mean Overall Ratings

TABLE 2

ALL PAU STUDENTS (NON-FOREIGN)		
	Male	Female
Pop Spanish	4.114	3.810
Mixed Spanish	4.081	4.236
Std. Spanish	4.419	4.351
Caló	2.685	2.837
Code-Switching	2.828	3.232

Mean Overall Ratings

TABLE 3

ALL PAU STUDENTS (NON-FOREIGN)			
		Male	Female
	Eval	4.445	3.998
Pop Spanish	Act/Pot	3.750	3.688
	Solid	4.564	4.074
	Eval	4.145	3.395
Mixed Spanish	Act/Pot	3.932	4.101
	Solid	4.327	4.529
	Eval	4.791	4.374
Std. Spanish	Act/Pot	3.864	4.231
	Solid	4.809	4.414
	Eval	2.236	2.681
Caló	Act/Pot	3.261	3.198
	Solid	2.145	2.238
	Eval	2.636	3.262
Code-Switching	Act/Pot	2.795	2.994
	Solid	2.891	3.464

Mean Evaluation, Activity/Potency, and
Solidarity Ratings

TABLE 4

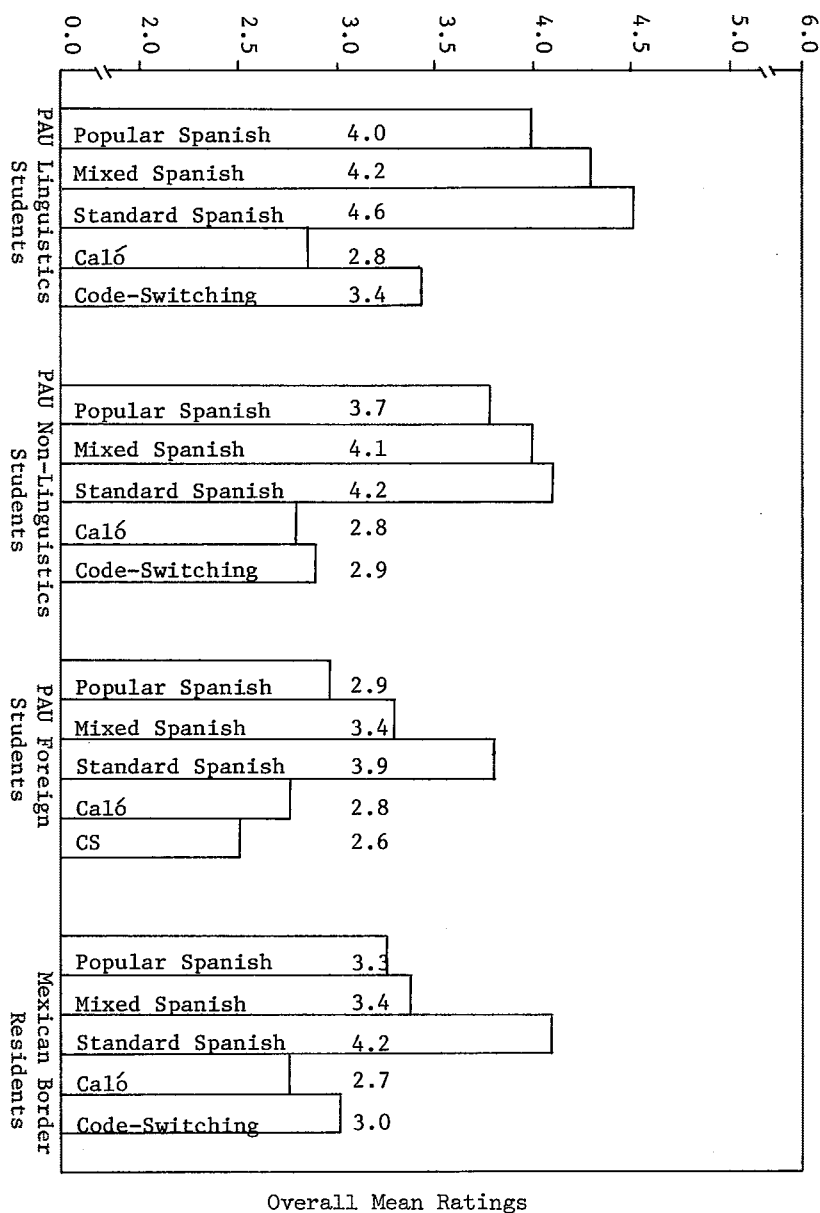


Figure 1

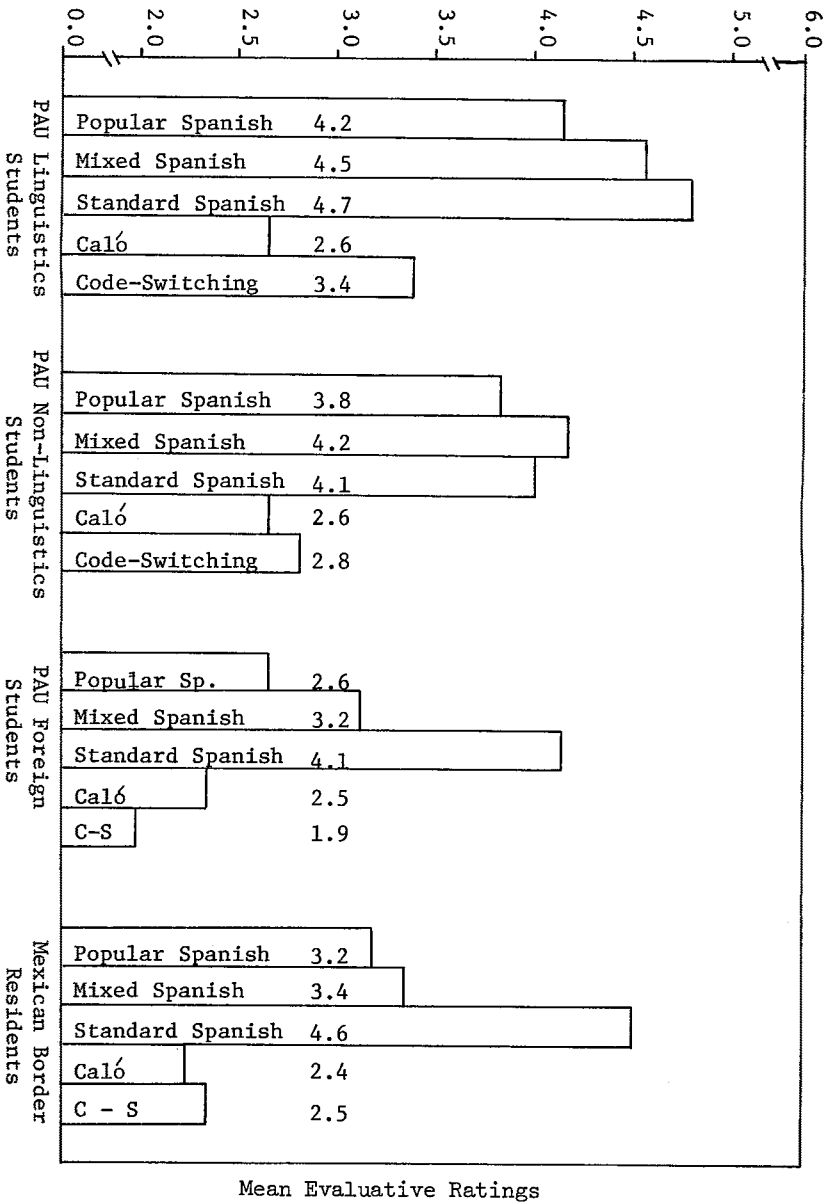
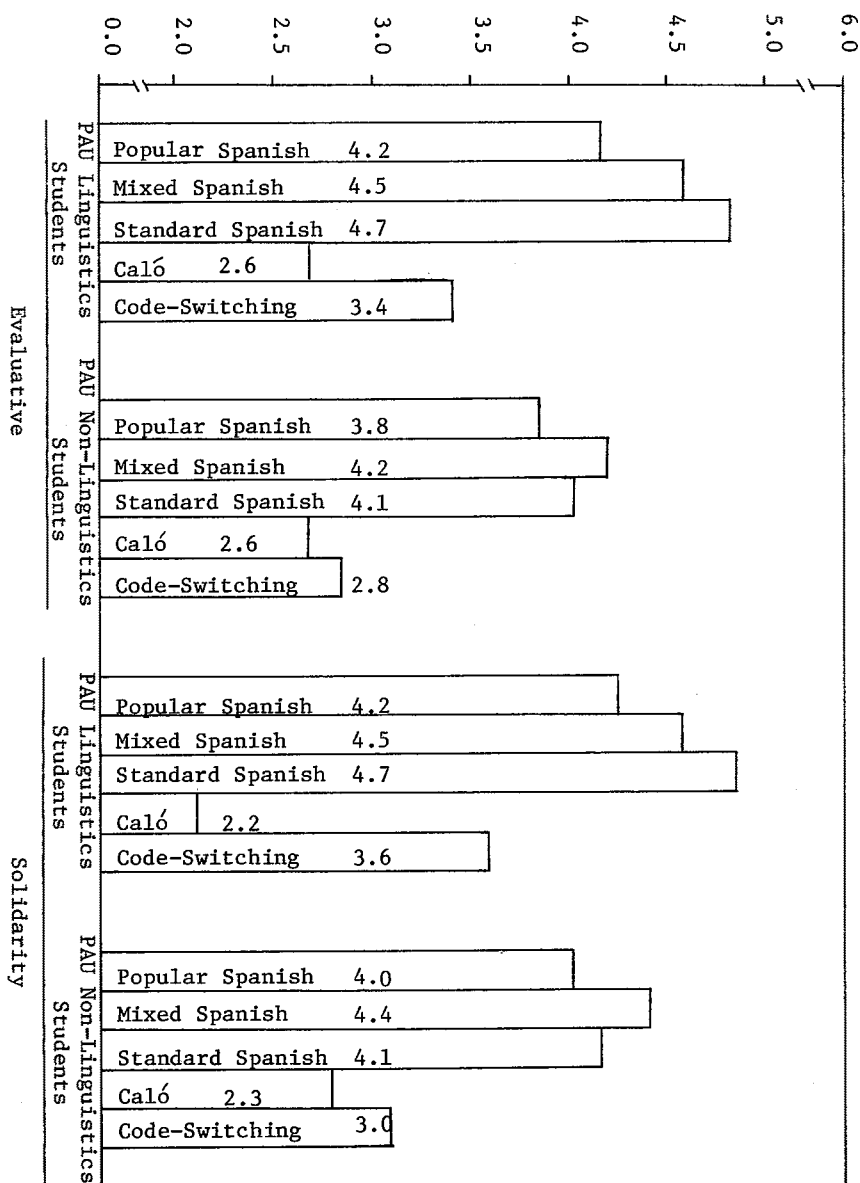


Figure 2



Mean Evaluative and Solidarity Ratings

Figure 3

REFERENCE

- Agheyisi, N., and J. A. Fishman. 1970. "Language attitude studies: a brief survey of methodological approaches." Anthropological Linguistics. 10(6): 137-157.
- Anisfeld, M., N. Bogo, and W. E. Lambert. 1962. "Evaluational reactions to accented English speech." Journal of Abnormal and Social Psychology. 65:223-231.
- Arthur, Bradford, Dorothee Farrar, and George Bradford. 1974. "Evaluation reactions of college students to dialect differences in the English of Mexican-Americans." Language and Speech. 17.3 225-270.
- Barker, G. C. 1974. "Social functions of language in Mexican-American community." Acta Americana. 4. 185-202.
- Carranza, M. A., and E. B. Ryan. 1975. "Evaluative reactions of bilingual Anglo and Mexican-American adolescents toward speakers of English and Spanish." International Journal of the Sociology of Language. In Press.
- D'Alejan, A. and G. R. Tucker. 1973. "Sociolinguistic correlates of speech style in Quebec." In R. Shuy and R. Fasold, eds. Language Attitudes: Current trends and prospects. Washington, D. C.: GUP. 1-27.
- Elías-Olivares, L. 1976. "Ways of speaking in a Chicano Community: A sociolinguistic approach." Austin: University of Texas dissertation.
- _____. 1977. "Un cuestionario sociolingüístico: Problemas en la interpretación de datos." In B. Hoffer and B. L. Dubois, eds, Southwest Areal Linguistics Then and Now, 217 -228. San Antonio, Trinity University Press.
- Galván, J., J. Pierce, and G. Underwood. 1976a. "Relationships between teacher attitudes and differences in the English of bilinguals." in Curt R. Douglas, ed. SWALLOW IV: Linguistics and Education. San Diego, CA: Institute for Cultural Pluralism/San Diego State University. 279-314.
- _____. 1976b. "The relevance of selected educational variables of teachers toward Mexican American English." Journal of LASSO. 2:1. 13-27.
- Gumperz, J. J. and E. Hernández-Chávez. 1972. "Bilingualism, Bidialectalism and classroom interaction." In C. Cazden, V. John, and D. Hymes, eds. Function of language in the classroom. New York: Teachers College.

- Hannum, T. 1975. "Attitudes toward Spanish: A field report." Paper presented at the Conference on Bilingual/Bicultural Education, El Paso, Texas.
- Lambert, W. E., R. C. Hodgson, R. C. Gardner, and S. Fillenbaum. 1960. "Evaluational reactions to spoken language." Journal of Abnormal and Social Psychology. 60: 44-51.
- Ornstein, J. and Paul Goodman. 1974. "Bilingual/Biculturalism viewed in the light of socio-economic correlates." Unpublished ms.
- Ornstein, J. and J. L. MacIntosh. 1974. "A brief sampling of West Texas Teacher attitudes toward South Spanish and English language varieties." Hispania V. 57.4 920-926.
- Osgood, C. E. and G. J. Suci. 1955. "Factor analysis of meaning." Journal of Experimental Psychology. 50: 325-338.
- Ryan, E. B. and M. A. Carranza. 1975. "Evaluative reactions of adolescents toward speakers of standard English and Mexican-American accented English." Journal of Personality and Social Psychology, 31.5 855-863.
- _____. 1976. "Ingroup and outgroup reactions to Mexican-American language varieties." To appear in H. Giles, ed., Language, Ethnicity, and Intergroup relations. New York: Academic Press.
- Shuy, R. W. 1969. "Subjective judgments in sociolinguistic analysis." In J. E. Alatis, ed. Linguistics and the teaching of standard English to speakers of other languages or dialects. 20th Annual Roundtable, No. 22 Washington, D. C. GUP. 175-188.
- Thompson, R. 1973. "Social correlates of regional pronunciation in Mexican-American English." Paper read at the 1973 summer meeting of the LSA.
- Tucker, G. R. and W. E. Lambert. 1969. White and negro listeners' reactions to various American English dialects. Social Forces 47: 463-468.
- Williams, F., J. L. Whitehead, and L. M. Miller. 1971. Attitudinal correlates of children's speech characteristics. (Final Report Grant No. OEO-0-70-7868 (508)). Austin, Texas: Center of Communication Research.

AZARBAYJANI: BILINGUALISM AND POLITICS

Reza Ordoubadian
Middle Tennessee State University

Azərbayjani belongs to the Southwestern group of the Turkic Languages, which is a branch of the Altaic and eventually Uralo-Altaic family of languages. Probably, the first speakers of this language came to the area at around 1060, and the language received relative acceptance during the reign of the Safavid Dynasty, a Persianized Turkic people, who ruled Azərbaycan as their home base as well as a large part of Iran between 1505 and 1736.

Azərbayjani flourished and expanded, and the land remained primarily under Persian rule until 1828, when it was partitioned into North Azərbaycan, annexed to the Tsarist Russia, and South Azərbaycan, remaining as a province of Iran.

Because of cultural-political reasons the literature and language of Azərbaycan could be divided into three periods: 1) from the beginning to 1828, 2) from 1828 to 1924, and 3) since 1924.

The first period is primarily Islamic in tradition and culture. The language was orally preserved; however, it was dominated by Persian and Arabic, and very little literature of consequence in Azərbaycan remains from this period.

During the nineteenth century and early twentieth century--the second period--Turkic literature shows a great diversity. New literary languages emerge. Most of the literary language of this period developed in the Tsarist Azərbaycan, and it reflects a great deal of various western influences. In the north the Tsarist Russia and the French influence the language. For this reason, a strong theatrical literature emerges. The plays of Akhundzadeh are considered, even after the Sovietization period, among the best-written and stageable products among the writings of the Azərbaycan people and the Middle East of the modern times. It is during this second period that a literary language, distinct from the ordinary oral language, develops in Azərbaycan; this is a vital phenomenon, because for any society to gain an identity, it must have a literary language.

In the third period--the Soviet period--there is a continuation of the literary forms of the second period; however, like the literature of other minority languages, the tradition

moves generally and primarily towards a standardized Great Russian tradition: it is only the "sound" of language that differentiates Azarbayjani from Russian.

During the first and second periods a modified version of the Arabic alphabet was employed for writing. This was, of course, a Persian influence. It is interesting to note that even after the Tzarist annexation of the northern part of Azarbayjan to Russia--during all of the nineteenth century and the first two decades of the twentieth century--up to the Sovietization period, this modified alphabet was employed both officially and in recording of the contemporary literature.

For a short period after the Communist revolution in Russia, Azarbayjan declared independence, and a tumultuous period of attacks and counterattacks ravaged the land. However, and meanwhile, the Mussavat Party, the party for the independence of the country, planned to adopt a modified Latin alphabet, because the Arabo-Persian script did not meet the needs of the language. By 1924, the date communism was established in Azarbayjan, this alphabet was in use, and for a decade and a half after the communists took over the government, this alphabet flourished and facilitated the writing of the language, because, as any newly adopted alphabet will do, it provided an almost phonetic writing of the language: no more written symbols without vocalization, no more distinction between three kinds of alveolar fricative(s), a residual from the Arabic language. Now the words/savab/ from the Arabic word ثواب (good will of God), /sʌndʌl/ from the Persian word صندل (chair) and/sʌvʌr/ from the Azarbayjani word سۆز (to love) will all be written with the /s/ phoneme.

In 1939, with a greater emphasis on the Great Russia and a move to Sovietize the remoter areas of the Republic, the Russian language was taught compulsorily, and thus the Cyrillic alphabet replaced the modified Latin alphabet. An improvement on the alphabet was made in 1959, and the present day Azarbayjani in the Russian Azarbayjan employs the modified Cyrillic.

An important point has to be made at this juncture. The Soviets insist on accepting the integrity of the Azarbayjani language, but at the same time they insist on writing the language in the Cyrillic alphabet, an unnecessary imposition. Obviously, the Cyrillic alphabet keeps the Russian language in focus, a devious manipulation of power politics. However, in the Iranian Azarbayjan the government simply ignores that such a language exists, so that Azarbayjani children are born and speak their mother tongue, Azarbayjani, yet the first day they arrive at their schools they are expected to know the Persian language as if they were born into it. As a result, people in the Iranian Azarbayjan grow up and speak to each other in Azarbayjani, yet when they correspond--and often when they produce literature--they employ the Persian language. Writing in Azarbayjani, in any alphabet, is greatly discouraged.

In the summer of 1970 I was researching in the Iranian Azarbayjan on a Fulbright Faculty Grant. I had all sorts of letters of introduction from the State Department, the Fulbright Committee, and the American Embassy in Teheran, and I was set to do some serious field work. My plans were to record several hours of tape from as many dialectal variations as possible. Of course, I found the people who, at first, tried to help me-- that is, before they knew of the nature of my research--were hedging and keeping their distance. I was puzzled until some brave soul suggested that I get a permit from the secret police, which I had no intention of doing. People were horrified that I was naive enough to attempt to make a study of a language that officially and politically did not exist, and every one said so. From the university professors to the common man in the streets, they were all afraid to have their voices recorded in Azarbayjani, because the official line is--and many documents have been produced to indicate the point--that Azarbayjani is only a dialect of Farsi, the national language. I did make my tapes, but every word was edited by the informants, and we erased much footage.

On the other hand, like the Greeks bearing gifts, the Russian generosity towards the Azarbayjani language has been politically motivated. As a matter of fact, at times, Azarbayjani has been sidetracked in favor of the Russian language. However, in spite of the Russian chauvinism, it has been politically expedient for Moscow to allow Azarbayjani a degree of freedom. The Russians still foster a pan-Turkic kind of possibility when Turkey and the Iranian Azarbayjan might join the Soviet Azarbayjan. They almost pulled through this dream in the late 1940's after the second World War when an independent Soviet-style Azarbayjan was created in the south, and had circumstances allowed, today there would have been a unification. So far as there is an official language that ties the Northern Azarbayjan to the Iranian Azarbayjan, there can be territorial claims. This will always ensure the continuation of the language.

One more point should be made at this time. Because of a continuous literary tradition and a compulsory education in the vernacular in Soviet Azarbayjan, the number of educated speakers, who are conscious of their language, is greater than in the Iranian Azarbayjan. In spite of this consciousness, many of the speakers in the Soviet Azarbayjan try to avoid identification with any one local dialect by deliberate use of spelling pronunciation, which, in many ways, is a norm rather than a linguistic reality, naturally developed through linguistic changes. For this reason, there is a strange uniformity in the language which belies natural changes in terms of assimilation, reduction, environmental phonological peculiarities connected with sound formation in the natural Azarbayjani dialects found in the rural areas and in most parts of the Iranian Azarbayjan. For example, in the Tabrizi dialect and rural Soviet Azarbayjani dialects,

the lexical item for the English word *none* is the sound sequence /hɛs/ from the Persian word /hič/. Of course, in the strict written pronunciation, the word will be /hɛč/, as in the Baku-educated dialect. Or the word for to *drink* is /iʃmax/, a Turkic word, but in the Baku dialect it becomes /iʃmɔk/.

What *Academie Francaise* could never do to the French language the Academy of the Azarbayjani language in Soviet Azarbayjan has been able to do to Azarbayjani. Normativeness in the language is a product of the academy. Of course, the Academy has been both a great help in the preservation of the language and also an obstacle in its natural development. It has helped the language through lexicography, literary prizes, rewarding of linguists, and compilation of precise and descriptive grammars for Azarbayjani. It also gives credence and respectability to the language, so that it is not completely drowned in the Russian language. On the other hand, it is inhibiting because the academy by normative rules tries to preserve the so-called "purity" of the language, thus disregarding the laws of the natural changes in any natural language. Politics play a tremendous role in the pronouncements of the Academy.

Some broad generalizations could be made about the contemporary Azarbayjani language.

The Azarbayjani language is an amazingly stable and uniform language; probably, in the past 900 years it has changed but little. Considering that the English language has changed from Anglo-Saxon, to Middle English, to Modern English during the same period of time, and noting that Anglo-Saxon is literally a foreign language for the present day English speaker, the stability of Azarbayjani becomes even more striking. There has been, of course, a steady infusion of primarily Persian vocabulary, and to a lesser degree Arabic words, into Azarbayjani. In the twentieth century the Russian language has made strong lexical advances in the Soviet Azarbayjan in such a way that, excepting the minor phonological divergences between the Iranian Azarbayjani and the Soviet Azarbayjani, lexical items more prominently account for variations in the two dialects. For example, the word for *beer* in the Soviet Russia is /pivɔ/, a loan word from the Russian language, whereas the Iranian Azarbayjani uses the Persian word /ab;jo/. In the north the Russian word for *train* is /poyezd/; in the south a French word via Persian, /tiren/, is used.

The Azarbayjani Language is stable in spite of the power politics that have marked the history of the development of the language, especially the political considerations influencing the treatment of the language by the Iranians and the Russians in the past 120 years, more particularly, since the Sovietization period. Probably, the language has remained stable structurally because it is highly agglutinative, and there is not much that

can be done with the limited morphemes that the Azarbayjani NP's and VP's contain. Vocabulary infusion only broadens the language, and phonological changes are closely tied to the nature of morpho-phonemes that constitute the elements of the sentence, that is, the agglutinative nature of the sentence. Short of banning the language from being used orally--in writing it is virtually and literally banned in Iran--it seems Azarbayjani will survive in spite of the political games played both by the Russians and the Iranians.

Bibliography

Farzaneh, M. A. *Principles of Azarbayjani Grammar*. Tabriz, Azarbayjan: Shams Publications, 1344 (1965).

This publication is in Azarbayjani and Persian.

Housholder, Frederick. *Basic Course in Azarbayjani*. The Hague: Mouton, 1965.

An excellent and the only English text available.

Karang, Abdol-Ali. *The Grammar of Contemporary Azarbayjani*. Tabriz, Azarbayjan: Mohammadi Elmiyeh, 1340 (1961).

This publication is in Azarbayjani and Persian.

The Turkic Languages and Literature of Central Asia, A Bibliography. Compiled by Rudolf Loewenthal. The Hague: Mouton. 1957.

Twenty-five hours of tapes from spoken Azarbayjani recorded in Azarbayjani under a Fulbright Faculty Grant in 1970.

WELL, ...
Carole P. Hines
Old Dominion University

In her article "Questionable Answers and Answerable Questions" Robin Lakoff (1973) discusses the introductory word well, as in (1):

(1) A: What time is it?

B: Well, I just told Bill that it was noon.

Her general concern is what makes an answer appropriate to a question. She accepts Grice's (1975) Cooperative Principle, which sets general conditions that apply to conversation, regardless of subject matter as in (2):

(2) Be as informative as possible.

Be truthful; have adequate evidence for what you say.

Be relevant.

Be clear.

When someone follows these rules, he is responding directly. But Grice notes that people can also communicate indirectly. When someone appears to be following the Cooperative Principle, and yet says something that violates these rules, the hearer can deduce how the response connects to the original remark. These deductions Grice calls Conversational Implicatures. Lakoff's point is that well is a signal that a rule is being violated -- in (1), the rule of relevance -- and thus alerts the hearer to make a deduction.

She then considers the use of well in questions (my numbering in the examples):

Well is found in questions in situations like the following:

- (3) (a) Well, did Harry capture the aardvark?
(b) Well, why isn't the garbage out?
(c) Well, didn't it rain yesterday, just like I said it would?
(d) Well, all right, Stanley, what have you got in your hand?

She concludes that with both answers and questions, well indicates some type of insufficiency; either the answer is not a complete answer, or the question introduced by well indicates an insufficiency in the previous utterance or action to which the question is a response.

She also considers the use of well in narratives; she cites as an example:

- (4) So the man went to the old witch doctor who lived in each of the pyramids, as he had been instructed. He went to one after another,

but none could help him. Finally, he reached the witch doctor who lived in the very last pyramid, and he asked him, "How can I get the silver screw out of my bellybutton?" Well, to make a long story short, the witch doctor who lived in the last pyramid went into his pyramid and came out with a little silver screwdriver. He inserted this in the silver screw that was in the man's bellybutton, and he unscrewed and unscrewed and unscrewed, and finally the screw came out.

She explains:

In narratives such as this, the speaker is using well to indicate that details have been omitted, that his narrative is not really complete.

Therefore, the generalization seems to be that well is used in case of an insufficiency in response, either by the respondent himself or by someone else.

Lakoff's notion of insufficiency is generally correct, but it is rather vague and does not explain why it is the word well that is used. In this paper I will propose another explanation, which still involves the Cooperative Principle, but which explains the use of well and connects it to the adverb well. I will use data from casual conversations, interviews, and literary sources to defend my proposal.

First, I assume that Grice's Cooperative Principle is right, that there are general conditions which, in one way or another, apply to conversation irrespective of its subject matter. I also assume with Grice that when a speaker responds indirectly to an utterance, the hearer, by working through the available data, deduces the connection. Conversational Implicatures produce what on the surface appear to be violations of the supermaxims of the Cooperative Principle but which in reality are purposive exploitations. The hearer, in deducing the information being implicated, must first of all assume that the Cooperative Principle is in operation.

In addition to the general Cooperative Principle, there are certain prescriptions that participants in a specific conversation are expected to fulfill. The response will be structured according to the specific form of the initial utterance. If the previous utterance is a question, the terms call for specific information in the response.

(5) A: What time is it?

B: Three o'clock.

An expressed value judgment will call for approval.

(6) A: Everybody but me cheated on the exam.

B: You're a good boy, Charlie.

The prescribed terms of the response may also be surprise, sympathy, admiration, expressions of comfort, etc. If a response is to be considered adequate, it must adhere to whatever terms have been prescribed. In addition it will follow a specific pattern.

Wherever a verbal response occurs it is made up of two things: (I) An acknowledgment of the previous utterance; an acknowledgment that it is all right for the other to speak, to say, or believe what he said. (II) The actual content of the response. When a participant in a conversation responds to an utterance, in whatever way, he is acknowledging the right of the previous speaker to make the utterance; his very response indicates that he concedes the right of the speaker to say it. When the content of the response accords with the prescribed terms of the previous utterance, the acknowledgment is tacit, i.e., the response includes the acknowledgment. However, when the content of the response fails in any way to fulfill the expectations set up by the previous utterance, the acknowledgment becomes explicit. Explicit acknowledgment can take the form of well. Well expresses the acknowledgment, and this acknowledgment is necessary because the response is not in the terms prescribed by the previous utterance. Since any response involves the two things, acknowledgment and content, there can be several possible forms of responses: acknowledgment (tacit), adequate response; acknowledgment (explicit), inadequate response; and no acknowledgment, inadequate response. The latter is exemplified in a response like Go to Hell or It's none of your business.

I will start with some simple examples to define possibilities, and then explore more complicated examples. Consider responses to questions. In (5) and (7) the response fulfills both the Cooperative Principle (assuming the person is speaking truthfully) and the specific requirements of the particular question: a time expression for (5), a yes or no for (7):

(7) A: Do you want some coffee?

B: Yes, Please.

Another case is the rhetorical question, in which only one answer is possible.

(8) A: Isn't this great?

B: Yes, it is.

Well does not occur in any of these responses; if it did, it would indicate a different type of response, as I will show shortly.

An answer can be slightly indirect, and yet its relevance be immediately obvious.

(9) A: How much do you charge for physical examinations?

B: The professional fee is \$60.

(10) A: How successful were you in your mail questionnaires?

B: Most people responded.

In strict logical terms, these are not direct replies. Well is possible, but unlikely, because in practical terms such responses are counted as direct responses.

An indirect response is as in (1) and (11):

(11) A: Could you lend me a dollar?

B: Well, I only have enough money for my lunch.

The well is an indication that the speaker knows he is not responding directly. The indirection may be caused by lack of knowledge, capability, or willingness. Note that leaving it out in (11) would make the response more abrupt. As a sign of acknowledgment, the well indicates that the person had a right to make the request;

without the well, the answer throws that right into question.

These examples have involved only the immediate verbal context of the question. But questions are not asked in a vacuum; the person who is questioned may infer that there are several specific assumptions behind the question. Consider (12) and several responses:

(12) A: Are you coming to the party?

B: Yes

B: Well, yes

B: Well, no

The first response is a direct reply. If, however, there is no good reason to ask the question (e.g., it is obvious that the person is coming to the party), the answer may be well, yes. The yes answer does answer the explicit question, but the well indicates that the person is not sure that he is responding to all that is meant in the question. In other words, this well can be a mark of puzzlement. If a person has not been invited to a party, the answer will be well, no. In asking the question, the first person assumes that the second was invited; the well indicates that this assumption is mistaken. In both cases, the well is required as a specific acknowledgment because the speaker can not respond fully to all the implicit requirements set down by the question. Similarly, in (5), an answer could be well, four o'clock if there was a clock visible to the questioner; the well indicates that the responder can not accept the implicit assumption that the clock is wrong (or the implicit situation that the questioner is too lazy to look at the clock).

Statements also set particular prescriptions for possible responses, as in (6) where approval is prescribed. Consider (13) and several possible responses:

(13) A: This article is extremely interesting.

B: Yes, it is.

B: I stayed up all night reading it.

B: Well, I don't think I really understood it.

The first response is direct; the statement asked for approval, and Yes, it is gives it. The second response is indirect; but it is approval, and in practical terms it fulfills the requirements of the statement. The third does not respond with approval; the well indicates that the first person may have a point, but that the second person cannot, for the reason given, support it. In (14) something of the same thing is true.

(14) A: "Are you sure they come from Denver?"

B: "Well," she said, "they told me they did."

The questioner is asking for verification, and the responder can offer only limited evidence; therefore, a direct response is impossible.

In addition, there may be a background situation that defines the need for a well. If a colleague of mine and I usually eat lunch at twelve-fifteen, and I walk into his office at twelve-twenty and find him intently working, I am likely to say "Well, I'm ready for lunch." If I had abruptly said "I'm ready for lunch," I seem to be ignoring him and his right to be doing what

he is doing. The well is a signal that I acknowledge his right, but since I use it, it also indicates that I cannot respond to the situation as he may like. In (15) a policeman is arresting two men who are trying to conceal a woman's purse.

(15) "Well, now! I've heard that purses for men were the 'in-thing!'" But somehow you guys don't look like the type." The policeman sees the men. His assessment reveals the discrepant information that the men are doing something that men don't ordinarily do, i.e., carry a woman's purse. The well is in response to this inadequacy.

If the well can acknowledge either the verbal or the non-verbal, Lakoff's comments on well used with questions are quite accurate. She is correct that there is an insufficiency involved; otherwise the well would not be present. But the well indicates only the acknowledgment, while the accompanying remark involves the insufficiency. (I will note later what happens when well occurs alone, as in well!) The same situation applies in narrative. In a story situation, the listener accepts the conditions set by the story -- not only its details of plot, but also, such things as its pace and degree of particularity. What happens in Well, to make a long story short is that these conditions are changed. The well acknowledges those conditions, and the following remark then specifies how they will be altered. The alteration is an "insufficiency" of sorts, especially in a shortened story, but well is also used, as I will show, in situations where a story has been interrupted, and the teller then resumes the story with a well to acknowledge the interruption and then continue.

It should be stressed that in its role as an acknowledging word, well is very general; there are several ways in which a person can accept what has been said before departing from it. The responder may accept the right of the other person to make the remark or ask the question, but nothing else; or he may accept the other person's opinion, while differing; or he may accept a statement in part, but not wholly; or he may accept it wholly, but wish to give mitigating circumstances. Such detailed possibilities are spelled out in other acknowledging phrases such as:

- (16) Be that as it may.
That may be true.
I see your point.
You're entitled to your opinion.
You're right, of course.
That's all well and good.

Each of these phrases is usually followed either by an explicit but or a remark that implicitly contains a but. The but, explicit or implicit, indicates how the Cooperative Principle is being violated -- or alternatively, how the following remark is discrepant with what is expected or required; or how the following remark is insufficient with respect to certain requirements.

We can consider the use of well in Stephen Crane's short story "The Open Boat." The story concerns four men shipwrecked off the coast of Florida. The four men are described as the

captain, the oiler, the cook, and the correspondent. They are in a small row boat trying to get to land, but unable to get too close to the shore. They consider ways in which they might get help from shore. The cook says that there is a house of refuge at Mosquito Inlet Light, and people there will see them and come out to pick them up.

"As soon as who sees us?" said the correspondent.

"The crew," said the cook.

"Houses of refuge don't have crews," said the correspondent. "As I understand them, they are only places where clothes and grub are stored for the benefit of shipwrecked people. They don't carry crews."

"Oh, yes, they do," said the cook.

"No, they don't," said the correspondent.

"Well, we're not there yet," said the oiler, in the stern. "Well," said the cook, "perhaps it's not a house of refuge that I'm thinking of as being near Mosquito Inlet Light; perhaps it's a life-saving station."

"We're not there yet," said the oiler in the stern.

The oiler intervenes with a well that does not take sides, and yet which disagrees with the point of the debate. The well of the cook seems to be conceding the correspondent's point, but it introduces a remark that changes the argument. As notable as the uses of well is its absence when it could be used, as in the correspondent's second comment; he could have said, "Well, houses of refuge don't have crews." Of course, disagreement does not break any of Grice's principles, but the use of well seems to assume a "Be agreeable" rule too. The lack of well in the correspondent's disagreement may be an indication that he considers any opinion of the cook not worthy enough to warrant a sign of acknowledgment. The lack of well is notable also in the oiler's repetition of his remark; he now seems to be impatient and not as willing to accept the argument.

Lakoff associated impatience with a use of well but here we have an example without it. Impatience appears to be a contingent quality of well. As a colleague of mine illustrated, well can be used in a dismissing manner, to stop discussion or change it; he was obviously impatient with protracted discussion of points he considered not worth discussing. But his impatience could also have been evidenced with the same accompanying remarks to well, but without the well.

(17) A: To understand well, you have to take the whole situation into account.

B: Well, I'm only interested in your main point.

B: I'm only interested in your main point.

The difference is one of politeness. The first answer seems to concede the plausibility of the first speaker's remark, while indicating a lack of interest in what would be the natural following information. The second answer seems to dismiss the plausibility. Both remarks indicate impatience; they differ in politeness. The politeness is signalled by a word that indicates some (perhaps small) measure of acknowledgment. We can see from this example

that well is well suited to be a mark of politeness, and it can be stereotyped as a politeness marker in many people's speech.

In another incident in "The Open Boat" the cook, described as the cheerful cook, notes the wild play of the waves and comments that it's a good thing that they have an onshore wind or otherwise they wouldn't have a "show." The oiler and the correspondent agree, and the captain laughingly asks if they think they really have much of a show anyhow. The three fall silent, and the captain, assessing the negative effect of his remark, attempts to comfort them with, "Oh, well, we'll get ashore all right." Lakoff's point applies here: Comfort is necessary when one feels insufficient, so expressions of comfort may often be accompanied by well.

A third incident occurs when they are near the Mosquito Inlet Lighthouse and come close enough to shore to see a building that they assume to be a life-saving station; but they can see no signs of human activity. The tide and the wind begin to swing them to the north. The captain responds to the situation, the weather and the lack of human help, by saying, "Well, I suppose we'll have to make a try for it ourselves." Note that in this case there is a sense of considered judgment attached to the well. All the particular connotations that occur in particular circumstances would suggest a highly polysemic word if we did not see the general use of the word, and understand how accompanying remarks can make its use appear so varied. The captain tells the oiler to head directly for the shore, but as the waves begin to break and roll over the boat, the oiler has to turn back out to sea again. In a combination of considered judgment and an attempt to comfort, someone says, "Well, anyhow, they must have seen us from the shore by now." Again, they are insufficiently able to cope with the situation.

In a fourth incident, someone sees them from the shore; they are at first hopeful, but then it becomes apparent that the person thinks they are simply on a pleasure boat ride. He waves his coat at them. They discuss this at some length at first because they think it is a signal. One of them says, "Well, if he'd just signal us to try the surf again, or go to sea and wait, or go north, or go south, or go to hell, there would be some reason in it, but look at him! He just stands there and keeps his coat revolving like a wheel. The ass!" The well here precedes an expression of impatience, but it can also indicate confusion about the situation. Often when the well is used as a signal of an inadequacy of response, the person feels his response to be inadequate because he does not understand how to respond, or why.

(18) A: I bet you know Tom Smith.

B: Well, yes, I do, but why do you want to know?

(19) A: Do you have a dollar bill with a red mark on it?

B: Well, I might. I'll look.

The use of well as a stereotyped indicator of politeness is strengthened by this circumstance. One aspect of politeness is that you never presume too much, but show that you are inadequate to the situation and willing to be given further information.

At dawn, the captain concludes: "Well, if no help is coming, we might better try a run through the surf right away. If we stay

out here much longer we will be too weak to do anything for ourselves at all." Again, we have an assessment of the situation. They swim through the surf, three of them making it, but the oiler drowns.

The use of well as a fictional device was probably heightened by uncertainty in "The Open Boat"; it created a situation in which people felt inadequate to respond to the demands of the situation. Another situation that lends itself to use of well is illustrated in Eudora Welty's story "Petrified Man."

The beauty shop setting of "Petrified Man" gives an air of light, inconsequential chatter. Ms. Welty, a master of realistic dialogue, uses well purposefully; every occurrence gives the reader a feeling not only for the movement of the story, but also the character and relationship of the participants in the dialogue. Since the story is one of continuous dialogue, well is used frequently, in fact, twenty-seven times, so I will only discuss several of the key occurrences.

The story begins as Leota, the hairdresser, prepares to shampoo Mrs. Fletcher's hair. She remarks that Mrs. Pike bought the peanuts that are in her bag.

"Who's Mrs. Pike?" asked Mrs. Fletcher, settling back. Leota's long, involved answer is an indication that she is totally prepared to conform with the requirements of the situation, i.e., gossip.

The first use of well occurs when Leota discovers that Mrs. Fletcher's hair is falling out, and Mrs. Fletcher suggests that the falling hair was caused by the last permanent Leota gave her. Leota denies this, and then Mrs. Fletcher asks if it is possible that she could have caught it from Mr. Fletcher.

"Well," Leota answered at last, "you know what I heard in here yestiddy,...--that you was p-r-e-g.- and..."

The question Mrs. Fletcher asks prescribes a yes or no answer, but Leota does not answer according to the prescribed terms because she is interested in two things: (1) shifting the blame for the falling hair onto Mrs. Fletcher herself and (2) verifying the gossip concerning Mrs. Fletcher's pregnancy. In doing these two things, she indirectly answers the question.

Another incident that illustrates several environments in which well is needed occurs when Mrs. Fletcher, still smarting from learning that practically everyone knows of her pregnancy, discovers a small child sitting under the sink. Leota explains that the little boy belongs to Mrs. Pike who has gotten a job at a millinery shop and is unable to keep him there because he keeps trying on the ladies' hats. Leota says,

"Here, he couldn't hurt a thing."

"Well! I don't like children that much," said Mrs. Fletcher.

"Well!" said Leota moodily.

"Well! I'm almost tempted not to have this one," said Mrs. Fletcher.

The first well, spoken emphatically, marks Mrs. Fletcher's unwillingness to agree with Leota, partially from pique and partially from her own feelings about children. Leota's forceful well is also revealing. She is faced with open disagreement with a customer if she does not back away with a direct answer from the attitude she has just expressed, i.e., an amused tolerance of children. She knows that if she openly disagrees, she may annoy the customer, but she also knows that Mrs. Fletcher is expecting a child. She lets well signal her unwillingness to agree, and she says it with enough force that what she doesn't say is quite apparent to Mrs. Fletcher. Mrs. Fletcher's response to Leota's unspoken criticism is equally strong. She, also, is unwilling to back away from her earlier statement so she makes a weak attempt to justify it. Here are three examples of well used with force. Notice that these differ from an introductory well, . . . These seem to be radically different uses but it is the same well; it is the voice inflection that makes the meaning difference.

In the following example, well also signals unwillingness to follow the terms prescribed by the relationship and the preceding utterance. Mrs. Fletcher, who is very concerned about her appearance, says,

" . . . and if I really look that pregnant already-:"

"Well, now, honey, I just want you to know - I habn't told any of my ladies and I ain't goin' to tell 'em - even that you're losing your hair"

In using well, Leota refuses to pacify Mrs. Fletcher's fears and gives reasons why she shouldn't be fearful. Giving reassurance, like this often consists of disagreeing with the fear expressed and so well is appropriate. Of course, Leota is not giving the reassurance that Mrs. Fletcher wants; Mrs. Fletcher wants Leota to say that she is not obviously pregnant, but Leota merely promises not to tell anyone.

My final example of the use of well in "Petrified Man" shows well used to continue a narrative that has been interrupted rather than to interrupt one. Leota uses it frequently in this way. At this point in the story she has been telling of a visit with Mrs. Pike to the freak show that has been running next door for the last few weeks. Mrs. Fletcher frequently interrupts to ask questions and to express disapproval. Leota responds to the questions and comments but always manages to move back to the topic of the freak show. She has just explained that at the freak show there are two dead babies that were born joined together and that the reason why they were born joined together was that the parents were first cousins.

"Me and Mr. Fletcher aren't one speck of kin, or he could never of had me," said Mrs. Fletcher.

"Of course not!" protested Leota. "Neither is me an' Fred, not that we know of. Well, honey, what Mrs. Pike liked was the pygmies . . ."

Well is used in a similar way in the following example from an interview.

(20) Reporter: But did you also become more prone to doing

laboratory work.
Doctor: Well, along with the physical tends to go
laboratory work. When a patient comes in
for a cold, there's not much laboratory
work that's indicated, but along with the
physical, there's a problem of high blood
pressure and there's a problem of heart
disease or something of that sort which
requires more laboratory work. I don't
think my percentage of laboratory work has
gone up but I may be wrong.
Well, come to think of it, probably it has
gone up because of doing more physicals -
more tests get done.

The expected response to this question is a simple yes or no and the doctor does eventually give the answer. The first well indicates that he knows he is not complying with the terms prescribed by the question, i.e., yes or no reply. He begins by conceding that physicals tend to create laboratory work. His attempt at justification ends in a negative answer to the question. The second well in the response shows that he recognizes the inadequacy of his own reply. He then gives the prescribed response. The inadequacy that he sees is not an insufficiency such as lack of information; it is a matter of logic.

Ms. Lakoff does not attempt to show a connection between well used as an introductory word and well as an adverb. The evidence presented here suggests that there is a relationship. Well is a word with positive connotations. It is used in an evaluative sense as an adverb to indicate a point just above adequacy. When it occurs as an introductory word, it also has positive connotations; it is an affirmation of the right of the previous speaker to say what he says. There is an insufficiency, but this is created by the circumstances in which well is used, not by well itself. Everytime we verbally respond according to the Cooperation Principle we give approval, but explicit approval is needed only when we are not responding exactly as prescribed.

Bibliography

- Crane, Stephen. "The Open Boat." An Introduction to Fiction.
Ed. X.J. Kennedy. Boston: Little, Brown and Company, 1976.
- Grice, H.P. "Logic and Conversation." The Logic of Grammar.
Eds. Donald Davidson and Gilbert Harman. Encino, California:
Dickinson Publishing Company, 1975.
- Lakoff, Robin. "Questionable Answers and Answerable Questions."
Issues in Linguistics: Papers in Honor of Henry and Renee
Kahane. Eds. B. Kachru, R.B. Lees, Y. Malkiel, and S. Saporta.
Urbana, Illinois: University of Illinois Press, 1973.
- Welty, Eudora. "Petrified Man." An Introduction to Fiction.
Ed. X.J. Kennedy. Boston: Little, Brown and Company, 1976.

TOWARDS A CLASSIFICATORY MATRIX FOR LINGUISTIC VARIETIES, OR:
THE DIALECT/LANGUAGE PROBLEM REVISITED

Wolfgang Wülc

State University of New York at Buffalo

1. Introduction

Scholars concerned with the study of language have long been using a number of terms and have been employing them to label what they obviously regard as prime concepts in their science. On the level of the internal structure of language as a system, two of the primary units that almost every linguist works with, are word and syllable. In the field of the comparative study of systems, and varieties of language, undoubtedly the two most frequently used terms are language and dialect. In studies of the social psychology of language, and in the new field of the study of the use of language in education which is one of the major, or perhaps the paramount concern of applied linguistics, a phenomenon very commonly referred to as cause, conditioner or correlate of others to be studied is that of bilingualism. You will probably wonder why I mentioned those five concepts: word, syllable, language, dialect and bilingual or bilingualism, in one breath when it seems obvious that some of them have little or nothing to do with one another. Truly, they are rather non-parametric, though they all share two important characteristics: on the one hand, they have all been profusely used in the study of language and its speakers, they have stimulated arguments and research and, if for no other reason, have been very useful, perhaps even helpful concepts, though on the other hand, none of them has ever been adequately defined.

Most definitions of bilingualism fall short of their goal because they avoid--explicitly (Haugen, 1956:9) or implicitly (Weinreich, 1954:1)-- the sensitive issue of the determination of the linguistic varieties involved in the contact situation. So far, little agreement has been reached among specialists, particularly on the delicate question of the language/dialect distinction, which has assumed embarrassing importance in the linguistic politics of some minority groups. Ostracizing the term "dialect" and substituting the generic "variety" for it only added to its stigmatization and made the specialist look more helpless (Editorial, The Linguistic Reporter, Jan. 1977).

2. Conventional definitions

Since the most obvious problems in the classification of linguistic varieties is that of identifying what is a language and what is a dialect let's start with that. After a look at the linguistic literature we are, unfortunately, not much wiser, largely because of the frequent fault of explaining one with reference to the other without having defined or explained that first. Those who try a somewhat independent definition, often employ the criterion of mutual intelligibility to identify both, that is dialects as linguistic varieties characterized by mutual intelligibility and a language as a variety composed of such mutually intelligible units. The primary concern of most classical definitions, by the way, is to identify what is a dialect rather than a language, apparently because the latter is considered less problematic. Mutual intelligibility as a criterion appears, e.g., in Sapir's definition of a dialect as "a form of speech which does not differ sufficiently from another to be unintelligible to the speakers of the latter (1931:124), and in Lehmann's "mutually intelligible forms of speech are known as dialects" (1964:3). De Saussure's definition of dialects is "idioms with only a very slight degree of divergence" (1916:264, translation mine) invokes the same, but Bloomfield seems to use a certain lack of mutual intelligibility as a defining criterion of dialects: "If linguistic change results in groups of persons between which communication is disturbed, these groups speak dialects of the language" (1926).

Obviously, divergence between linguistic systems, even if 'slight', as Saussure puts it, impairs intelligibility at least to some extent. Which extent is permitted to preserve 'mutual intelligibility' is very difficult to establish unequivocally, though at least one successful attempt in the right direction is known (Casad, 1974). Without clear enough criteria, even this seemingly reliable measure appears rather fuzzy. Even the best attempts at distinguishing language or dialect contain fuzzy terms, that is, terms which put the burden of proof on the reader's imagination, such as Saussure's quantifier 'slight' or Sapir's 'sufficiently' in the two recent quotes. Other such terms are 'normal', 'broad enough', 'substantial' which appear, e.g., in Ferguson & Gumperz' definitions (1960:5ff). If intelligibility testing was intended as a shortcut to get an idea of linguistic divergence, for which we apparently have not yet any other reliable measures, it may serve that purpose, namely to give us an idea, but very little more.

Besides logical circularity and fuzziness, I consider as another common flaw of many current attempts the insistence on purely linguistic criteria for the distinction between the two varieties as there are, e.g., the calculus of rules of grammar, number of isoglosses, or number of identical lexemes. There is no harm in trying to push the available tools of our discipline as

far as possible. The danger, however, lies in a form of scientism which believes that a subject matter must be explainable exclusively by the tools of our limited sub-discipline simply because that phenomenon happens to reach partly into its realm. This tendency easily leads people into the temptation of arbitrarily picking some criterion and establishing 'ad hoc' operational definitions, such as a certain number of isoglosses for 'their' dialect unit. This rather debatable method has at least the advantage of being checkable or verifiable, although communication and cross-comparisons become near impossible if everyone means something else by the same term.

3. A new approach.

In this rather confused situation a new approach to the problem seemed to be justified. Based upon the realization that these so-called linguistic varieties are sociolinguistic realities of usage I decided several years ago to approach their users and elicit their definitions, not only for the two most problematic ones but also for a few others in the popular domain of usage, such as accents, jargons and even, where applicable, such problem terms as 'pidgin' or 'creole'. Many colleagues and students have helped me in this endeavor and have contributed material from parts of the world outside my own scope of experience. My own data came primarily from Germany, Britain, Ibero-America and the U.S., with spot checks in Romance and Scandinavian Europe.

The purpose of these interviews, most of them informal, was to establish a series of identificational characteristics rather than differential ones for the varieties listed, i.e., to define the respective variety first by what it is rather than what it is not or how it differs from others. Thus we avoid the problem of circularity, but permit uni-dependency, i.e., the occasional use of a previously defined variety in the definition of another. To anticipate some of the results I can announce right now that we found a surprising amount of agreement among our respondents' characterizations, -- far, far, more than we ever get among us specialists.

Here, then, are, first of all, the characteristics of language and dialect as abstracted from our interviews. Since dialect has traditionally been more interesting, let's start with it: Dialects are identifiable by their 'tone' or 'melody', sounds, words and also by expressions and constructions. In more linguistic terms this means: intonation, phonology, lexicon, morpho-syntax; or, in other words, they have unique characteristics on all levels of structure. Significantly, syntactic or constructional peculiarities are mentioned seldom for dialects, a point I'd like to come back to, perhaps, later. Nevertheless, dialects have complete grammatical systems, a fact by now perhaps too obvious to most of you to belabor any further, other than to add regrets that some colleagues of superficial egalitarian persuasion have taken this as conclusive evidence for the equivalence of language and dialect. The main

distinguishing feature of a dialect, however, is its orality, the fact that it is spoken. That does not preclude the possibility of using graphic symbols to represent it; usually these are adapted to it from the orthography of the closest written language, be it related or simply geographically close-by, however with a wide range of variation among its authors or users. A necessary correlate of its use in oral communication is the restriction of the communicative spread of a dialect to only regional diffusion across communities of rather limited size. Lastly, a person whose communicative repertoire consists of linguistic competence in a dialect only, is generally found at the lower end of the socio-economic scale.

A language, of course, also has its specific grammar, as almost every child knows or is told soon enough. Here, however, grammar refers particularly to its morphosyntax and lexicon and, very significantly, to the conventions governing its orthographic representation. In its phonological representation, that is when spoken, there is, on the other hand, plenty of room for local or regional and social variation, known as accents. One could, therefore, say that it lacks phonological specificity. Its most frequently repeated defining characteristic is that it has a tradition of writing, most easily attestable in certain orthographic conventions. People are easily able to name subdivision of it, either in terms of accents which subdivide its spoken usage, or even as dialects "of that language" of which local or regional accents are often the last trace (cf. Span. 'dejo'), particularly where they, i.e., the dialects, have either lost their linguistic homogeneity or, perhaps, have not yet developed it (cf. Bloch's (1948) definition of a dialect as: "a class of idiolects with the same phonological system", which duly reflects the situation of U.S. American English where 'dialect' and 'accent' are often used interchangeably). To continue with less purely linguistic or sociolinguistic parameters: a language has national or even international diffusion, and at least those who use it 'properly' are those who have enjoyed the benefits of a formal education, that is they belong to the higher strata of the socio-economic scale.

4. The European prejudice, or: standardized languages.

It is quite obvious that the foregoing description fits one particular representation of the variety language, namely a standardized language. I am not at all sure, however, that everybody realizes that (1) the socio-linguistic conditions required for the development of, as Garvin (1959) put it, the 'elaboration' of standard languages are far from being the rule for the world's many languages and that (2) the traditional language/dialect dichotomy is unthinkable, if not inconceivable for most people--and I suspect even for some linguists without their necessarily being aware of it--without the implicit assumption of the existence of a supra-regional standard for the glottopolitical purpose of wider communication and as a kind of super structure or physically tangible roof over the dialects of that language to which they are

then attributable. Witness the following statement in Louis Gray's *Foundations of Language* (1960:28): "After the development of a standard language, the several dialects which each contributed to create it...generally...steadily decline in dignity, until they are spoken only by the lowest strata of society within the language area; their use may even be discouraged as a mark of ignorance; those who still speak them may seek both to drop them themselves and to induce their children not to use them." And under the entry 'dialect' in Pei and Gaynor's *Dictionary of Linguistics* (1960:56) we read: "A dialect is...a specific form of a given language, spoken in a certain locality or geographic area, showing sufficient difference from the standard or literary form of that language as to pronunciation, grammatical construction and idiomatic usage of words, to be considered a distinct entity, yet not sufficiently distinct from other dialects to be regarded as a different language."

I already mentioned that the elaboration of standard language forms depends upon very specific sociolinguistic or socio-political conditions of power, industrialization and technocracy which generally lead to a very strictly stratified society typical, e.g., of most of Europe and its derivatives. We could easily say that the dialect/language problem is culturo-centric or endemic to such socio-linguistic or political situations and conclude our diagnosis here. The trouble, however, is that this very specific conceptual model is always transferred into situations in which it is not applicable, namely into socio-linguistic situations frequently encountered in the so-called Third World, although they are not confined to it, as the cases of many European languages, e.g. Welsh, Basque, Friulian and a great many North American ones, easily attest. Again, we could simply declare these cases out of bounds for our classificatory attentions and decide to avoid any conflict by outlawing the conventional terms and revert to the generic term 'variety' for safety's sake until further notice, i.e., until further socio-political development of those language groups. This, in my opinion, could be the only sensible interpretation of, e.g., Troike's editorial in the January 1977 number of the *Linguistic Reporter*, entitled: What's in a name? The trouble, however, is that for the ethno-cultural groups in those post-colonial or neo-colonial polities where their own forms of speech are dominated or marginalized by so-called majorities and their standards, linguistic and otherwise, there is a lot in a name, often enough to risk their lives for it. I believe that these people can expect something more of us than a sort of benevolent resignation. Probably because the word 'variety' does not have any linguistic meaning for most people it is unlikely to become very popular, other than in uses as the following: "Dialects, like the poor are always with us. Attempts to eradicate them and standardize the language often meet with signal success, but new dialects will then arise out of the unified, standardized language. Like other local differences of food, dress, and customs, dialects are often a nuisance. Yet they lend picturesque variety to language, and variety is the spice of life."

(Pei, 1966:47, underlining mine.)

Invariably, people will fall back on the terms of the European model and there the term dialect offers itself with enough good reason not to call its user simply stupid or discriminating. Since orality, as I mentioned before, seems to be the most significant feature of the dialect unit, the primarily oral use of many speech varieties of the Third World invokes this categorization. The next question that then arises in such situations in accordance with the European model is: what language is it a dialect of? Language is here taken clearly in the sense of standard language, because quite often we are annoyed when some ignoramuses seriously think that e.g., Quechua, in Andean South America is a dialect of Spanish whose standard dominates that language area. If you have ever heard certain bilingual dialects of Quechua and, for that matter, of Spanish, you might become a bit more tolerant of this misinterpretation. Linguists, of course, seldom make such mistakes. They, however, might commit one whose ethno-cultural consequences could be far-reaching if their suggestions were taken too seriously. Some of them offer this seemingly very attractive alternative to the stigmatized 'dialect' label given to each tangible representative, i.e., regional dialect, of such varieties as Quechua which I just mentioned: They propose to call each one a language of its own. This is not so much a case of egalitarianism as cited earlier. There are two interrelated good reasons for that interpretation: The first is the occasional lack of immediate mutual intelligibility particularly between geographically distant dialects; and secondly or rather as a cause of the latter, these dialects have a lot more structural homogeneity and systemic specificity than their often rather debilitated typological correspondents who just barely survived the powerful normative and levelling influence of a standardized supra-regional form.

5. Another kind of language

Our principal method of eliciting folk definitions for the concepts language or dialect from the users themselves, characteristically, cannot be applied in such instances, because they do not seem to have a generic term for the 'dialect' variety other than proper names of locations or regions. What we get most often is equivalent to 'speech' or French 'parler', Spanish 'habla', German 'Sprache', Quechua 'rimay', or refers to significant parts of the articulatory organism, like tongue, mouth, as in Quechua 'simi'. Much more frequently you get the proper name for the variety used by the entire ethno-cultural community, like the name 'Quechua'. This in itself is already significant of an awareness of a linguistic unity above the regional dialect level, which is also reflected in Halliday's institutional definition of a language community as "a group of people regarding themselves as using the same language." (1964:76). This awareness of the internal cohesion of such a linguistic variety which is only manifested in the form of often rather disparate dialects and which lacks any tangible supra-regional form is clearly testable and verifiable,

as we have done in the South American situation. Confronted with a recording from a distant dialect a speaker of Quechua will not hesitate to admit that this sounds a lot like he should be able to understand it though he cannot quite make it all out; while a recording from even a more adjacent dialect of another language, say Campa or Aymara will elicit a totally different response. In other words, there is hardly any question about the outer limits of such a language, much less so than in many cases of standard national languages and their dialects, where national borders often do not at all coincide with geographical linguistic boundaries as, e.g., between Italy and France or between Holland and Germany. The significant overt characteristic of this ethno-cultural pre-standardized language, which I have called external distinctiveness or internal cohesion in the matrix, is, perhaps, the more easily observable manifestation of a deeper conceptual awareness of the structural commonalities shared in some way by all the dialects of such a language. In a first attempt at finding these commonalities, I have recently undertaken to deduce or synthesize from very detailed and independent descriptions of six representative dialects of Peruvian Quechua some of those characteristics which can be said to be fundamental to at least Peruvian Quechua if not the whole Quechua language (Wölck, in press.) Hymes' suggestion to identify functions first and then to look for the formal means that fulfill these functions, was a good guide in my endeavors. The result was a surprisingly systematic, recurrent set of functional categories shared by all dialects which make their often seemingly so disparate formal physical realizations in different dialects look so much less different, remote or puzzling. I can, of course, not guarantee that this method of functional categorization will establish the same underlying linguistic unity in similar cases, but I think it is very likely and the process is certainly worth a try. This way, at least, we can justify the status of such varieties as languages beyond any doubt and in their own right, without the necessity of a supra-regional standard or its written form.

I shall conclude by pointing to the obvious chronological relationship between ethno-cultural, pre-standardized languages, which for ease of reference I will call Type I languages, and the socio-political, standardized Type II languages. In most cases the existence of the former is a prerequisite for the development of the latter, given the necessary political conditions. The process is, however, not always unidirectional only, as shown by cases like that of Welsh or Low German, which had developed some sort of an administrative standard when their linguistic communities were also politically autonomous but lost it and subsequently regressed into their present states of great dialect diversity or re-dialectalization.

The appended matrix shows how the three linguistic varieties of central concern in this presentation, *viz.* dialect, pre-standardized and standardized language, are characterized by the

features discussed in the paper. A number of other varieties or the (English) names for them are also entered and identified in the matrix to show its potential for classificatory purposes. I would certainly not claim that this is a final list of features or characteristics sufficient to identify all possible varieties, and I regret that time does not permit me to elaborate here on the varieties beyond the three of major concern for this paper. Perhaps we can get to some of them in the discussion period.

References

- Bloch, B. (1948) "A set of postulates for phonemic analysis." *Language* 24:3-46.
- Bloomfield, L. (1926) "A set of postulates for the science of language", *Language* 2: 153-64.
- Casad, E. H. (1974) *Dialect intelligibility testing*, Norman, Okla. (Summer Institute of Linguistics).
- Ferguson, C. A., & J. J. Gumperz, eds. (1960) *Linguistic diversity in South Asia*, IJAL 26, 3,3. Bloomington, Ind. (Research Center for Anthropology, Folklore and Linguistics).
- Garvin, P. L. (1959) "The standard language problem: concepts and methods", *Anthropological Linguistics* 1,3:28-31.
- Gray, L. H. (1960) *Foundations of Language*, New York (Macmillan)
- Halliday, M.A.K., (1964) "The users and uses of language", in M.A.K. Halliday et al., *The linguistic sciences and language teaching*. London (Longmans Green), 75-110.
- Haugen, E. (1956) *Bilingualism in the Americas: A Bibliography and Research Guide*. University, Alabama (American Dialect Society.)
- Lehmann, W. P. (1964) *Historical Linguistics: an introduction*, New York (Holt, Rinehart & Winston).
- Pei, M. (1966) *The story of language*. Allen & Unwin, London.
- ____ & F. Gaynor (1960) *Dictionary of linguistics*, London (Peter Owen)
- Sapir, E. (1931) "Dialect", in *Encyclopedia of the Social Sciences*, vol. 5, pp. 123-6, New York.
- Saussure, F. de (1916) *Cours de linguistique generale*, C. Bally & A. Sechehaye, eds., Lausanne (Payot).
- Troike, R. C. (1977) "What's in a name?", *The Linguistic Reporter* 19,4:2.
- Weinreich (1953) *Language in contact: findings and problems*, New York (Linguistic Circle of New York).
- Wólck, W. (in press) *Quechua general: algunos replanteos*. Lima (Instituto de Estudios Peruanos).

<u>Symbols:</u> ✓ = applies + = present - = absent () = or <i>circled = necessary</i>	Grammar			Contact between:			Subdivisions		Users (class)				Uses				Diffusion			
	phonology	lexicon	all levels	interference	Languages	Dialects	External distinctiveness/ internal cohesion	lower	higher	one	all	oral	written	special	all	community	national	multinational	international	
					fusion															
ACCENT, FOREIGN	(+)			(✓)								✓								
ACCENT, LOCAL	(+)					(✓)						✓				✓				
JARGON		(✓)								(✓)				(✓)						
DIALECT			(✓)			-		✓				(✓)				(✓)				
LANGUAGE I, PRE-ST.			(✓)			(+)	(✓)	(✓)				✓					✓	(✓)		
LANGUAGE II, STAND.	-					(✓)			✓				(✓)				(✓)		(✓)	
CREOLE			(✓)		(✓)		✓				✓				✓					
PIDGIN			✓	(✓)						✓		✓								

V

**LINGUISTIC METATHEORY:
EPISTEMOLOGICAL FOUNDATIONS OF LINGUISTICS**

AN EMPIRICIST EPISTEMOLOGY FOR LINGUISTICS¹

Paul L. Garvin

State University of New York at Buffalo

I.

From the outset of my career, I have had a feeling of malaise with the prevalent theoretical trends in American linguistics: both with the earlier behaviorist orientation, and the more recent generativist emphasis.² It is only in the last few years that I became aware of the depth of my disagreement - it is concerned with the very mode of thinking exemplified by these theoretical positions. In order to clarify and justify my own point of view, I found it necessary to render explicit the fundamental premises of my conception of language and consequently of linguistics. In other words, I found it necessary to render explicit my epistemological position.

At its very core is the view of language as an integral part of human social behavior. An epistemology for linguistics in keeping with such a broad view of language must therefore be derived from an epistemology applying to the behavioral sciences at large.

There are two approaches to the epistemological foundations of the behavioral sciences of which I am aware at present. On the one hand, there are the philosophers of science whose concern includes the behavioral sciences. Their argumentation is most often based on the experience of the natural sciences which they regard as the model for a scientific discipline. They derive the epistemological principles from the theory and practice of the natural sciences and apply these same principles to the behavioral sciences. On the other hand, there are the social and behavioral scientists interested in developing epistemological foundations for their respective fields. Their major concern seems to be with the principles that are required to make their areas of study into "genuine" sciences, that is, again like the natural sciences. I find both of these modes of thinking unsatisfactory, for two reasons:

(1) a basic misunderstanding of the nature of behavioral patterns: the patterns of human social behavior are both more complex and have "fuzzier edges" than the principles governing the functioning of the physical universe.

(2) a basic misunderstanding of the notion of science - science is defined in terms of a particular set of techniques developed on the basis of the experience of the natural sciences (such as experimental method, formal theorizing, or the use of

statistical tools),³ rather than in terms of the most appropriate theoretical and methodological way of dealing with the particular data that are confronted. Thus, the basic aim for an epistemology for the behavioral sciences is that it be appropriate to behavioral data rather than conform to a preconceived notion of what science should be like - particularly if such a notion stems from an experience that is essentially alien to that of the behavioral sciences.

My epistemological position is chosen on the basis of the metatheoretical assertion that formal models are inappropriate for a behavioral science. I call it metatheoretical because it represents my theoretical view of what a theory should be like. By formal theories I mean theories that have formal mathematical or logical pretensions, such as proposals based on set-theoretical notions or on propositional calculus offered as models for the representation of human social behavior, linguistic or otherwise. It seems to me that human social behavior is far too subtle and complex to be realistically represented by such models; I cannot see how equations, mathematical functions, logical propositions or similar formal tools can adequately encompass the indeterminacies, intricacies, "fuzzy edges" and general complexity of naturally occurring human behavior.

A metatheoretical assertion represents the basic value judgment underlying the development of an epistemological position: the assertion voiced here clearly attributes the highest value to realism rather than to some ideal such as elegance or simplicity. I strongly feel that undue emphasis on elegance and/or simplicity may easily lead to a distortion of reality, and my basic aim is to hold distortion to a minimum.

I am therefore proposing that a mode of theorizing appropriate to the analysis of behavioral data has to be based on an epistemological position conforming to the metatheoretical assertion voiced above.

II.

I believe that in order to provide an adequate specification of an epistemological frame of reference, three key notional components have to be stipulated: a guiding principle, a set of initial assumptions, and a set of conceptual tools.

The guiding principle voices the choice of the basic explanatory principle or principles to be used in an epistemological frame of reference. It thus differs from the metatheoretical assertion (see end of Section I) which prescribes the choice of a mode of theorizing.

The initial assumptions are the broad general assumptions which represent the investigator's basic beliefs about the nature

of the object of study. Being broad and general assumptions, they are necessarily vague; representing as they do basic beliefs, they necessarily convey a strong reality claim (i.e., a strong claim of corresponding to reality).

The conceptual tools constitute the conceptual apparatus that is necessary in order to formulate and answer the detailed questions that pertain to the research intended to lead to an analytic understanding of the object of study. Being concerned with details, the conceptual tools are necessarily specific; being in the nature of questions, they necessarily convey a weak reality claim.

Before detailing my epistemological orientation in terms of the frame of reference presented above, I should like to consider a number of crucial points concerning it.

1. In my opinion, the commitment to a particular epistemological orientation as expressed by a metatheoretical assertion and a guiding principle is a matter of the investigator's individual choice. Thus, a researcher may - depending on personal conviction, rational argument, adherence to fashion or whatever other motivation - choose a rationalist, behaviorist or other epistemological orientation.

2. The key notional components in terms of which I propose to stipulate an epistemological frame of reference are in a logical order of priority: the choice of a guiding principle will determine the choice of initial assumptions; and the choice of initial assumptions will in turn determine the choice of conceptual tools.

3. Of the three key notional components, the guiding principle is the most general. The initial assumptions and conceptual tools, on the other hand, are more specific. Each consists of a multiplicity of notions - the former are fewer in number than the latter, since clearly, there are fewer basic beliefs than there are questions addressed to the data. For the same reason - basic beliefs vs. questions to be asked - the initial assumptions are designed to serve an entire broad field of study while the conceptual tools must be adapted to particular more specific problem configurations. The three key notional components of an epistemological frame of reference can thus be considered three levels of epistemological specification, going from the most general and most strongly held convictions (the guiding principle) to the most specific and most loosely held (the conceptual tools).

They represent clearly different confidence levels in terms of the reality claims they imply, and must therefore be rigorously distinguished.

4. In my personal approach the initial assumptions and the

conceptual tools are organized into two types of models. The initial assumptions are organized into a single theoretical model applicable to all of language and all languages. The conceptual tools are organized into a series of reference models of varying degrees of specificity, each of them designed for a particular aspect of language and particular types of languages at various levels of detail (phonological, morphological, syntactic reference models, as well as various typological reference models within each). The relation between the two types of models is that the reference models serve to "flesh out" the skeleton provided by the theoretical model by furnishing the necessary detail.

5. One of the crucial questions regarding an epistemological orientation is the level within the frame of reference at which hypotheses in the technical sense come into play. In most theoretical approaches, attempts are made to derive testable hypotheses directly from the theoretical model. In my own approach, neither the theoretical model nor the various reference models serve to generate testable hypotheses immediately. Rather, a methodological strategy is developed on the basis of the two types of models, and a large number of small-scale hypotheses are derived in the course of the application of the detailed operational steps prescribed by the strategy (see Section VII).

In summary, then, my epistemological position is characterized by the following points:

(a) The basic value judgments underlying an epistemological orientation are made explicit, rather than being buried under a contention that "complete objectivity" can be achieved.

(b) Two types of models (a theoretical model and an array of reference models, representing clearly distinct confidence levels) are proposed, rather than including all epistemological notions in a single model in which distinctions of confidence level of necessity must be blurred.

(c) Hypotheses in the technical sense are considered to be part of the methodological, rather than the theoretical apparatus, since they emerge in the process of applying a method rather than being part of the theoretical foundations of the research.

III.

The guiding principle that I am espousing is that of functional empiricism. As the term implies, there are two aspects to this guiding principle: an empiricist aspect and a functionalist aspect. Let me discuss these in turn.

The empiricist aspect of the guiding principle stems from the requirement that all assumptions about the nature of the object, as well as about the nature of the method used to deal

with the object, should be derived from the observation of behavior and should be susceptible of being referred back to this observation. For linguistics this means that all assumptions about the nature of language, as well as about the nature of linguistic method, must be derived from the observation of the behavior of native speakers, and be susceptible of being referred back to it.

The empiricist aspect of the guiding principle further implies that the path of inference from data to conclusions should at all times be made explicit in all its aspects. It also means that the abstract system underlying the patterning of a given form of human social behavior, including language, should be inferred from its concrete manifestations; thus, in the case of language, its abstract system can (and must) be studied through its concrete manifestations in the behavior of native speakers. The record of this behavior, the corpus, represents a sample of the infinity of possible utterances that can be produced in a speech community. The adequacy of this sample is an empirical question which can be answered by empirical means.

This is the opposite of the more commonly held principle that the properties of the object should be deducible from a predesigned theoretical model - in the present approach, the theoretical model together with the reference models serve as the frame of reference for developing a guided inferential process leading from data to conclusions, that is, an orderly discovery procedure. This, in my view, allows one to achieve a cumulative approach in which it becomes possible to differentiate clearly between analytic results on the one hand and mere assumptions or opinions on the other.

As can be seen, the empiricist aspect of the guiding principle is strongly concerned with the inferential path by which the abstract system underlying the patterning of a given form of behavior is arrived at; it is not, on the other hand, strongly concerned with the ontological status or the locus of this underlying abstract system. Thus, the question is not raised as to whether this abstract system represents a structure lodged in the mind - or in the brain - of the participant in the behavior, or whether it "merely" represents a systematization of the investigator's observations about regularities in behavior - such different ontological interpretations are not relevant because they do not help to answer the basic question of the manner in which an understanding of the abstract system is arrived at.

The functionalist aspect of the guiding principle asserts that a structure (such as that of language or any other behavioral system) must be studied in terms of its functioning. This assertion requires a further clarification of the notion of function.

The term "function" is here not used in a formal logico-

mathematical sense but in an informal humanistic sense. The informal question is raised as to how the system as a whole functions and how its elements function with respect to each other. In order to deal with these questions adequately, the informal notion of function as used here has to be further specified as follows:⁴

1. Since the notion of function is here applied both to the functioning of a system as a whole and to the functioning of the elements with respect to each other, a distinction is implied between two kinds of functions, namely, the external functions (those characterizing the functioning of the system as a whole), and the internal functions (those characterizing the functioning of the elements with respect to each other).

Of the two, only the external functions currently serve as explanatory principles in functional empiricism, and hence the subsequent discussion will be limited to the latter.

2. Function in the sense of external function can be equated with the role that a given behavioral system plays in the life of the people. This means that function is not the same as domain of use, but rather it has to be inferred from the conditions affecting the use of the system in a given domain.

3. It seems to be necessary to distinguish between immediate and ultimate functions. What is meant by this is the difference between the immediately observable role of a certain behavioral system as opposed to its "deeper" significance. The latter is often not at all clear, and may even appear to contradict the former; thus, the immediate function of a given form of behavior may be attention-getting, while the ultimate function may be tension reduction.

4. The same function may be carried by more than one system or mode of behavior. Thus, for example, the same meaning (i.e., referential function - see below) may be conveyed by more than one channel of communication - as for instance, a greeting which may be conveyed verbally or by a gesture.

5. While a given system of behavior may have more than one function in the sense discussed above, often only one of these functions is dominant for the particular system, while the remaining functions may be considered subordinate. Thus, for instance, the referential function (the function of referring to various types of phenomena distinguished by the culture - grossly corresponding to what is sometimes called 'intellectual meaning') may be viewed as the dominant one for a system such as that of natural human language, while the esthetic function (that is, the function of provoking an esthetic response - which in turn may be defined in a variety of ways, but that definitional problem need not be considered here) may be viewed as the dominant function in

artistic systems such as those of music or certain of the graphic arts. The distinction between dominant and subordinate functions is of importance, since it is reasonable to assume that a given behavioral system is structured in terms of its dominant, rather than its subordinate, functions.

6. Finally, it should be noted that (with some conspicuous exceptions such as the referential function of language) the functions of a given behavioral system are far from obvious. This means that in most cases the particular functions of a system under study should not be postulated beforehand as part of the epistemological apparatus, but rather should be discovered through the application of the methodological apparatus - that is, in the light of the empirical aspect of the guiding principle, they should be inferred from the observation of behavior.

7. In the case of linguistics, the referential function (as already noted under 5 and 6) can be considered the dominant function in terms of which the structure of language may be characterized, as will be detailed in Section V below.

In summary, functional empiricism can be characterized as an event-centered rather than a person-centered approach (Frederick O. Gearing, personal communication), since it is directed towards the explication of the nature of behavioral events rather than that of some attribute of the human person (such as, for instance, cognition).

IV.

Let me now turn to the initial assumptions that follow from the guiding principle of functional empiricism. In doing this, let me stress again that they represent assertions of the investigator's basic beliefs and hence tend to be general and vague, but make a strong reality claim.

In the case of linguistics, the vagueness of the initial assumptions is basically justified by the requirement that they account for the two apparently contradictory conclusions that emerge from any serious consideration of the variety of human languages: one is that all the languages of the world are similar to each other in that they all are languages; the other is that all the languages of the world are dissimilar to each other, in that they all are different languages. Any theoretical framework must account for both the similarity and the dissimilarity in such a way that they become logically compatible. A theoretically useful conception therefore has to be specific enough so that it stipulates all the necessary properties of a natural language; it also has to be general enough so that it can accommodate all the variety of different characteristics of particular languages that can be uncovered through linguistic analysis.

Since, as I noted at the outset, I view language as a behavioral system, the initial assumptions about language should conform to whatever broader initial assumptions can be made about behavioral systems in general. Consequently, before detailing the particular initial assumptions pertaining to language I shall first outline briefly some of these broader initial assumptions.

Most of these assumptions will appear obvious. They are, however, far from trivial if their consequences are taken seriously and followed up in detail. In the present section, these assumptions will be stated and their consequences for the case of language will be briefly commented on. The details of the theoretical model that I have been proposing for linguistics will be discussed in Section V.

1. Human social behavior is complex. This assumption is more often than not disregarded in the process of model-building in linguistics and other behavioral sciences. In general terms, it implies that the criterion of simplicity is of itself not adequate for the evaluation of models (or, for that matter, results) in a behavioral science; it also implies that it is unrealistic to expect that the patterning of human social behavior could be adequately represented by a model based on a single explanatory principle. More specifically with regard to linguistics, this assumption implies that the system of language is structured in not just one direction, with a single set of levels ascending from, say, phonology to, say, syntax or semantics, as set forth in the American descriptivist tradition (see Trager 1963) or, more recently, in stratificational grammar (see Lamb 1966). Rather, the system of language is conceived as a multiple hierarchy in the sense of being structured in more than one direction.

2. Human social behavior is systematic. This assumption is necessary to justify the claim that human social behavior is amenable to an orderly description - if it were wholly random, no such description would be conceivable.⁵

At least two questions are raised by this assumption.

A. How many systems are involved in a particular form of human social behavior? In the case of language, the question can be rephrased to ask how many subsystems there are within the system of language, and how these subsystems relate to each other. This question is clearly related to the notion of multiple hierarchy.

B. What is the nature of a given behavioral system? In the case of language, I follow the Saussurean view that language is a system of signs, with all the consequences that this conception entails.⁶

3. Behavioral systems are ill-defined systems. What is meant by this assumption is that the indeterminacies of human behavior are an intrinsic part of the behavioral systems and that consequently such systems have to be conceived of in such a way as to allow for these indeterminacies. Ill-definedness thus implies that behavioral systems are by definition open-ended, that they have "fuzzy edges," that "to every rule there may be an exception."⁷ In summary, the ill-definedness of the system means that the possibility of unsystematic residues is within the theoretical model considered an intrinsic property of behavioral systems, rather than a consequence of what might be called "our inadequate knowledge of such systems" with the ultimate hope that with advancing knowledge these indeterminacies could be removed and behavioral systems be presented in a closed and well-defined fashion.

This assumption is closely related to the assumption about the Gestalt principle to be stated under 5 below, which implies such ill-defined phenomena as overlappings and mergers; it is also implicit in the metatheoretical assertion discussed in Section I, in accord with which formal models - which are essentially representations of well-defined systems - are not appropriate to a behavioral science.

4. Patterned human social behavior exhibits a co-variance between form and function. This assumption is a direct consequence of the functional aspect of the guiding principle. By form is here meant the observable aspect of behavior events, by function is meant their underlying significance in the sense in which the notion of function was discussed further above. By co-variance is meant a correlation of the sort that any change in the observable aspect of a given behavior event can be expected to entail a change in its functional significance, and conversely.

Given that, in the case of language, the referential function is the dominant one (see under 7 in Section III), the form/function co-variance here is expressed through the form/meaning co-variance. This is a further development of the Saussurean notion that the sign in general (and the linguistic sign in particular, cf. also under 2 above) is "une entité psychique à deux faces" (1955:99). My interpretation of this notion is that the two-sided nature of the linguistic sign manifests itself in appropriate ways throughout the different aspects of language. Thus, I do not accept the notion of a separate semantic (or sememic) level (or component); rather, each aspect of language displays its own manifestation of the form/meaning co-variance.

Note that co-variance here does not imply a one-to-one correspondence. On the contrary, in the light of the metatheoretical assertion voiced in Section I and of the assumption of the ill-definedness of behavioral systems discussed under 3 above, the form/function co-variance is assumed to be imperfect: more

than one event may have the same function; the same event may have more than one function.

In the case of language, this is exemplified on the one hand by the notion of allomorphy (more than one form can be allomorphs of the same morpheme by virtue of sharing the same meaning), on the other hand by the many examples of individual morphemes having more than one grammatical meaning (e.g., the well-known case of inflectional affixes in Indo-European languages having more than one category meaning).

As with the preceding assumption, so with the present one a number of questions arise, of which one can be considered of particular importance to the case of language. This is the question as to whether form and function (in the case of language, form and meaning) are of equal significance or whether one of them can be considered more relevant than the other. Or, to use a different term, can one of them be considered the dominant co-variant? In my view the answer is yes: in the light of the functionalist aspect of the guiding principle discussed further above, function can be considered the dominant co-variant in the form/function co-variance - in the case of language, meaning the dominant co-variant in the form/meaning co-variance.

There are several arguments in support of this contention, some theoretical and some operational.

The theoretical argumentation is derived more or less directly from the already mentioned functionalist aspect of the guiding principle. The basic argument is that a behavioral event cannot be considered significant unless some function in the sense discussed earlier can be discerned to relate to it. In the case of language, this means that any of its characteristics, general or particular, can be considered to be linguistically significant to the extent to which they are pertinent to the expression or differentiation of meaning. Thus the phonemic and morphemic levels of the grammar are in my theoretical model distinguished in terms of the differentiation (phonemic) and conveyance (morphemic) of meaning.

The operational argument in favor of function being the dominant co-variant is related to the assumption that in order to understand the structure of behavioral systems it is necessary to establish properly defined analytical units of behavior - in the case of language, properly defined linguistic units. And in my frame of reference, meaning is an essential ingredient in the process of defining these units.⁸

5. Patterened human social behavior exhibits the Gestalt principle. What I have in mind here is the notion of Gestalt in the broadest sense, that is, "that the whole is more than the sum of its parts," rather than the mechanics of technical Gestalt

psychology. This broad conception of the Gestalt principle implies that the units of behavior are not usually viewed as hermetically sealed off from each other - rather, it is reasonable to assume that the boundaries between the units of behavior may overlap; that units of behavior will fuse into more complex units with overall properties of their own, rather than mechanically follow each other like beads on a string.

In the case of language, this means, firstly, that boundaries between linguistic units are not always clearly marked but are subject to fuzziness and overlappings, as illustrated by the complex morphophonemics of morpheme overlap such as colloquial English "whyncha" for more explicit "why don't you." It means, secondly, that linguistic units obey a principle of integration - they do not constitute mere sequential strings, but merge into fused units with Gestalt qualities of their own - the most conspicuous phonological example of this is the fusion of phonemes into syllables; another conspicuous example taken from the lexicon is the fusion of several terms into a single lexical unit the meaning of which is unrelated to the sum of the meanings of its constituents. Thus, the term "dove-tailing" has very little to do with either "doves" or "tails."

Finally, language more than many other behavioral systems also exhibits the figure/ground relation which is another manifestation of the Gestalt principle, much discussed in the Gestalt psychology of perception. The principle involved here is that figures (that is, outlined shapes) are perceived in terms of a ground (that is, the particular background against which they are outlined) - consequently, the same figure will be perceived differently when set off against different grounds. In the case of language, this means that linguistic units are perceived against the background of the linguistic context and extralinguistic setting in which they occur, and both their formal and their meaning characteristics are affected by this background. The most conspicuous example of the effect of the linguistic context is the variation of the phonetic shape of phonological segments according to their phonetic context; the most conspicuous example of the effect of the extralinguistic setting is the varying perception of the meaning of a given lexical unit under different cultural and situational conditions.⁹

6. There are different degrees of awareness with respect to different behavioral systems or different aspects of a given behavioral system. What is meant by this is that the extent to which participants in a behavior are aware of the presence of a given behavioral system or a particular aspect of a given behavioral system may vary, and that this variation may well be a characteristic of the particular system or aspect of the system. The notion of degrees of awareness seems to have both theoretical and operational significance. Theoretically, the degree of awareness appears to be related to something like degrees of

psychological "depth," in the sense in which clinical psychologists use the term. Behaviors of which the participant is more aware seem to be in some way closer to the psychological "surface"; behaviors of which the participants are less aware seem to be "deeper" in a psychological sense.¹⁰ The operational significance of degrees of awareness is related to this: clearly, behaviors of which the participants are aware to differing degrees will require correspondingly different analytical approaches - thus, it is not unreasonable to expect that a behavior of which the participant is clearly aware can be discussed in a framework of direct questioning, while behaviors of which participants are not clearly aware do not lend themselves to this type of interviewing.

In the case of language, the difference in degrees of awareness relates particularly to the distinction between lexicon and grammar made in the present frame of reference. As noted further below, theoretically lexicon and grammar are considered two basically different aspects of language - the two dimensions differing from each other, among others, in terms of levels of awareness: the lexicon exhibits a high level of awareness, while the grammar exhibits a low level: "[naïve native speakers'] awareness of lexical phenomena is immediate, untaught, and in most cases overt, as can be seen from responses such as 'This is the way we call it', or 'We have no name for it', or again 'A chair is a kind of seat'. By contrast, the awareness of grammatical phenomena on the part of the same native speakers is only potential in the sense that the recognition of these phenomena has to be taught." (Mathiot 1967:709). Although it is not unreasonable to assume that there may be some relation between this and psychological "depth" in the sense discussed above, this question still remains to be adequately explored. The difference in levels of awareness also is of great operational significance in the present approach to linguistics: the lexicon and the grammar require significantly different methodological treatment (cf. Mathiot 1973.24-33).

V.

The theoretical model of language which I have been proposing¹¹ is intended to stipulate the general properties of natural languages in the light of the broader properties of behavioral systems discussed in the preceding section. Such a theoretical view of language is in fact more of a definition or set of definitions in the classical sense than a model in the usual formal sense. Hence, I have been calling it a definitional model.

As all definitions, the definition of language should include a genus and a differentia specifica. The genus portion of the definition must set forth those properties which natural language shares with other comparable objects of study and on the basis of which it can be included in a larger class of phenomena. The

differentia specifica portion of the definition must then set forth those properties which make natural language a particular species of this larger genus of phenomena and differentiate it from other related objects of study within the same class.

The genus portion of the definition of language stipulates that language is a system of signs in the Saussurean sense (cf. under 2 in the preceding section). The notion of system of signs also implies the notion of form/meaning co-variance (see under 4 in the preceding section).

The differentia specifica portion of the definition of language stipulates, firstly, that the system of language is ill-defined (see under 3 in the preceding section), and secondly, that it is a multiple hierarchy (see under 1 in the preceding section). The latter will be further elaborated here. Specifically, language is considered to be a system structured in two dimensions and along three planes, with a separate set of levels proper to each of the planes. Each of these are characterized in functional terms.

First, the dimensions. Language has two dimensions: those of the grammar and the lexicon. The two dimensions differ in terms of the way in which the dominant function of language, the referential function (see under 4 in the preceding section), manifests itself: in the lexicon, the referential function is expressed as a naming function - lexical units serve primarily as labels (see Mathiot 1973.22-3); in the grammar, it is not.

The stipulation of a separate lexical dimension is a relatively recent facet of my theoretical position. The necessity for such a stipulation has been established by Mathiot's work on lexicology (1967.708-10, 1973.19-22), but the structure of the lexical dimension has not yet been sufficiently explored to permit a detailed formulation in terms of my theoretical frame of reference.

The grammatical dimension, on the other hand, is well enough known to permit a theoretical formulation of its structure. It is characterized by three planes, each with its own set of distinctions.

The first of these planes is the plane of structuring. It is characterized by two levels of structuring, those of phonemics and morphemics. The second plane is that of integration. It is characterized by an unspecified number of levels of integration. The third is the plane of organization. It is characterized by two organizing principles, those of selection and arrangement.

All of these distinctions are defined by functional criteria:

- (1) The two levels of structuring differ in terms of the

extent to which the units of each level participate in the sign function (meaning) of language. The units of the phonemic level function primarily as differentiators of the sign function, the units of the morphemic level function as its carriers.

(2) The levels of integration differ in terms of the order of complexity of the units that constitute them: they range from the level of minimal units, which is the lowest, to the level of the maximal fused units, which is the highest. Fused units are considered to be not mere sequences of units of a lower order, but to function as entities of their own order, with certain overall qualities above and beyond the mere sum of their constituents, thus manifesting the Gestalt principle discussed under 5 in the preceding section.

A correlate of the concept of fused units is the conception that the internal structure and the external functioning of a given unit are separate and potentially independent characteristics: units with the same internal structure may have different external functioning; units with different internal structure may have the same external functioning.

Units with the same internal structure are called identically constituted; units with the same external functioning are called functionally equivalent.

(3) The two organizing principles on the plane of organization characterize different manners in which the signaling means of the language are employed: selection from an inventory versus arrangement in a sequence.¹²

The three planes of the grammatical dimension of language are in a hierarchical relation to each other. The plane of structuring is defined by the most significant functional criterion and is therefore superordinate to the other two planes. Of the latter, the plane of integration is in turn superordinate to the plane of organization. Consequently, within each level of the plane of structuring a set of levels of integration can be defined, and within each level of integration of either level of structuring the operation of both organizing principles can be discerned.

The theoretical model outlined here serves as a frame of reference for the development and justification of the reference models and methodological strategies needed in linguistic analysis, as follows:

(1) The concept of the separateness of the two dimensions of language provides the justification for limiting the description to one of the two dimensions; thus, it allows the treatment of the grammar without including the lexicon, and conversely.

(2) The concept of the levels of phonemics and morphemics

on the plane of structuring requires the differentiation of the description of the phonemic pattern from that of the morphemic pattern, and to deal with their interrelation as a distinct aspect of the description.

(3) The concept of the levels of integration requires the organization of the description in terms of both minimal units and various orders of fused units, on both the phonemic and morphemic levels of structuring.

Thus, on the phonemic level, it requires a description not only in terms of minimal units such as individual phonemes (and their features), but also in terms of whatever phonemic fused units -- such as syllables, breath groups and units of still higher order of complexity -- are present in a given language.

(4) The concept of the potential independence of internal structure and external functioning requires the differentiation of these two aspects of structure within the description.

(5) The concept of the organizing principles of selection and arrangement on the plane of organization requires the inclusion, in the description, of the statement not only of inventories of units of all kinds (both minimal units and fused units), but also of their distribution (i.e., their pattern of occurrence, including such considerations as mandatory co-occurrence and selection restrictions).

Each of the above principles is equally pertinent to both reference models and methodological strategies, as will be further discussed in Sections VI and VII.

VI.

Let me now turn to the third of the key notional components of my epistemological frame of reference, the conceptual tools. As already noted, the conceptual tools differ from the initial assumptions in that they represent the analyst's intuitions about what are reasonable expectations concerning his object of study, rather than a set of firm beliefs. More specifically, the conceptual tools are those notions that are required to formulate and answer the detailed inquiries that must be directed at the data in the course of the analysis in order to produce a description. These inquiries relate to the important parameters of the object of study, which in the case of language concern primarily its units and the relations between them.

A number of additional points noted earlier deserve repetition and further elaboration. The first of these has to do with the systematicity of the conceptual tools; this means that the conceptual tools are organized into an array of reference models; conversely, it can then be said that a reference model consists of

a systematic collection of conceptual tools (see under 4 in Section II).

The second point is that, while a single theoretical model -- given its greater generality -- may be adequate to encompass an entire system of behavior, the specificity of the conceptual tools and the need to relate them to particular inquiries of detail requires that they be organized into more than one reference model, each of them dealing with a different aspect of a system under study (see *ibid.*); this will be elaborated further below.

The third point is that the reference models do not represent a theoretical conception of the nature of the system under study, but rather an operational frame of reference for the conduct of the analysis, designed with a view towards providing highly specific points of reference for the strategy of the analysis (see under 3 in Section II). In order to serve this purpose, the reference models are designed with a high degree of regularity and consistency, in clear violation of the initial assumption of the ill-definedness of behavioral systems (see under 3 in Section IV). Thus, the reference models are from the outset viewed as highly idealized collections of conceptual tools with no pretense of representing the reality of the structure for the analysis of which they are to serve. This reality is not expected to emerge until after a description has been provided, and is from the very outset expected to deviate from the reference models, since no behavioral system can be expected to conform to the high degree of regularity and consistency with which reference models are designed.

The fourth point is that reference models are not hypotheses or subtheories (see under 5 in Section II). That is, they do not make claims as do hypotheses or theoretical constructs, but ask questions. For this reason, they are not subject to either confirmation or disconfirmation -- rather, they are frames of reference for asking questions about the data in a systematic way, subject to modification rather than to confirmation or disconfirmation. If for a given case (such as for a given language) the answer to a particular question is negative (or the question is inapplicable), the reference model is not thereby invalidated (though it may have to be modified), since that same question may have a positive answer, or be applicable in some other case (such as for some other language).

In the case of language, reference models have been developed on the basis of the recognition of the possibility (and sometimes necessity) of differences in the way in which the various aspects of language (i.e., its dimensions, planes and levels) are structured, as well as of differences in the way in which different languages are structured (i.e., their typological diversity is admitted and stressed). These differences, of course, require different analytic frames of reference both for the different aspects of language and for typologically different languages;

hence, the need for more than one reference model.

Thus, the following variables affect the selection and design of the reference models used in grammatical analysis:

- (1) The levels of structuring and the levels of integration;
- (2) The typological characteristics of the language under investigation as revealed by various key analytic steps.

The two levels of structuring require different analytic treatment; hence, a separate general reference model is designed for each of them: a reference model of phonemic structure and a reference model of morphemic structure. Within each of these two general reference models, a number of specific reference models are designed to deal with the expected levels of integration within each level of structuring.

Finally, typological characteristics may serve to divide each specific reference model into two portions. The first of these is a common reference model applicable to all languages; this reference model specifies certain universal characteristics and serves as the frame of reference for the first steps of the analysis until the typological characteristics of the language under study emerge. The second is a series of typological reference models, the appropriate one of which is chosen for a given language after its assignment to a particular language type as a result of the application of the key step in the analytic procedure that is designed to reveal a typologically significant characteristic.

Given the stress on typological diversity noted above, corresponding stress is placed in the present approach on the development of typological reference models.

VII.

As already implied under 5 in Section II, the purpose of developing an empiricist epistemology for the study of behavioral systems is the creation of a systematic methodological apparatus for such a study; in the case of linguistics this has been traditionally called linguistic analysis (and more recently also discovery procedure).¹³

The aim of the methodological apparatus is to uncover the patterning of a particular exemplar of the object of study -- in the case of linguistics, the structure of a particular language. Note that, in terms of functional empiricism, the grammar (or the lexicon) of a particular language is not a theory of that language, but a description based on a theory and organized in terms of the theoretical model and of the reference models applicable to the language in question (see Sections V and VI).

It is expected that such an approach will insure a substantially higher degree of reliability and replicability than one in which the methodological apparatus is less thoroughly explicated.

Viewed in this perspective, a systematic empirical method is a heuristic rather than an algorithmic process.

An algorithmic process is one in which all of the steps of the process are clearly defined ahead of time and in which the success of the operation is in a very real sense guaranteed beforehand -- a classical example of such a process is the algorithm for long division. An algorithmic process can usually be applied only to the solution of clearly delimited and fairly straightforward problems such as are encountered in arithmetic or other well-defined areas of study.

A heuristic process, on the other hand, does not guarantee success as does an algorithm. It does, however, allow itself to be applied to less clearcut and more complex problems -- clearly, the study of language and other behavioral phenomena falls into this latter category (particularly in the light of the initial assumptions regarding their complexity and regarding the ill-definedness of behavioral systems discussed under 1 and 3 in Section IV).

A heuristic process is characterized by a flexible strategy which consists in the judicious manipulation of trials -- "stabs" at the data -- and evaluation of the results of each trial.

I have developed, and am in the process of writing up (see Garvin in preparation) a detailed discovery heuristic for languages of a particular type, namely, those characterized by a position-class morphology and an "accusative" rather than an "ergative" syntax.¹⁴ This heuristic can be considered a particular typological branch of a more general branching heuristic ultimately to be developed for the analysis of languages of different types.

The basic structure of this heuristic follows the frame of reference provided by the definitional model (see end of Section V), and utilizes the appropriate array of reference models (see Section VI) for the design of the strategy of the typological branch that has been developed. The individual trials and evaluations appropriate to the different facets of the analysis are implemented through the design of suitable analytic step sequences. It is in the course of the application of these analytic step sequences to a body of data that testable hypotheses in the technical sense are developed and can be verified.

VIII.

In conclusion, I want to underline the extent to which the epistemology of functional empiricism is nonformal and unspecific.

Only thus can it conform to the metatheoretical assertion voiced in Section I.

In a behavioral science such as linguistics, the aim of a particular approach cannot be to make "predictions" in the natural science sense. Rather, it is to provide a frame of reference for a description of the object of study, as well as to provide operational controls for the many variables that must be manipulated in the course of the analysis. In the approach presented here, the general frame of reference is provided by the theoretical model, and the operational controls are provided by the reference models and the discovery heuristic employing them.

FOOTNOTES

¹This is an adaptation to linguistics of an earlier, more general paper (Garvin 1977). The author has benefited greatly from suggestions by Madeleine Mathiot.

²Bar-Hillel (1967:420) spoke of my "individualism and [my] reluctance to join the crowd and to become a second fiddle in one of the fashionable linguistic orchestras".

³For a philosopher's critique of the usual epistemological approach to the behavioral sciences, see Kaplan 1964.

⁴For a detailed discussion of the notion of function, see Mathiot and Garvin 1975.

⁵As Hjelmslev (1953:24) puts it for the case of language, "A process is unimaginable - because it would be in an absolute and irrevocable sense inexplicable - without a system lying behind it."

⁶Some of these consequences were discussed as early as the late '20ies and early '30ies by Karl Bühler (see especially 1934; for a summary statement of his pertinent views, see Garvin 1966).

⁷Cf. Hockett's discussion of language as an ill-defined system (1968:44-55).

⁸Note the relegation of meaning to the status of "makeshift" in earlier American empiricist approaches (cf. Bloch 1948:3).

- ⁹Note that in other approaches the fusional aspect of the Gestalt principle is sometimes accommodated by indirection through notions such as "bracketing" or "embedding," while the setting-derived aspect of the figure/ground relation is relegated to "pragmatics." In the approach advocated here, on the other hand, the Gestalt principle occupies a central place. This means, on the one hand, the rejection of notions such as "string analysis" or "concatenation algebra," and on the other hand, a conception of linguistics broad enough to include the setting-related as well as the context-related aspects of language.
- ¹⁰Note that this distinction is of a different order than that between deep structure and surface structure made in some current approaches to linguistics.
- ¹¹For earlier presentations of this model, see Garvin 1963, 1969.
- ¹²Cf. the paradigmatic and syntagmatic axes in the European structuralist tradition; see Martinet 1960.33.
- ¹³For the classical negative view of this, see Chomsky 1957.50-53.
- ¹⁴For an earlier statement of some facets of this discovery heuristic, see Garvin 1972.

REFERENCES

- Bar-Hillel, Yehoshua, 1967. Review of Paul L. Garvin, *On Linguistic Method; Selected Papers*. Foundations of Language 3.420-8.
- Bloch, Bernard, 1948. A Set of Postulates for Phonemic Analysis, Language 24.3-46.
- Bühler, Karl, 1934. Sprachtheorie, Jena, Gustav Fischer.
- Chomsky, Noam, 1957. Syntactic Structures (=Ianua linguarum, series minor 4), The Hague and Paris, Mouton.
- Garvin, Paul L., 1963. The Definitional Model of Language, in Paul L. Garvin, ed., Natural Language and the Computer, New York, etc., McGraw-Hill, pp. 3-22.
- Garvin, Paul L., 1966. Karl Bühler's Contribution to the Theory of Language. Journal of General Psychology 75.212-5.
- Garvin, Paul L., 1969. The Role of Function in Linguistic Theory Actes du Xe Congrès International de Linguistes, Bucharest, Editions de l'Académie de la République Socialiste de Roumanie, Vol. I, pp. 287-90.

- Garvin, Paul L., 1972. On Linguistic Method (=Ianua linguarum, series minor 30), 2nd ed., The Hague and Paris, Mouton.
- Garvin, Paul L., 1977. Functional Empiricism, An Epistemology for the Behavioral Sciences. Mimeo.
- Garvin, Paul L., in preparation. Discovery Procedure: Theory and Practice. An Empiricist Epistemology for Linguistics and its Application to the Writing of a Grammar.
- Hjelmslev, Louis, 1953 [1941], Prolegomena to a Theory of Language, transl. by Francis J. Whitfield. Indiana University Publications in Anthropology and Linguistics, Memoir 7. Baltimore, Waverly Press.
- Hockett, Charles F., 1968. The State of the Art (=Ianua linguarum, series minor 73), The Hague and Paris, Mouton.
- Kaplan, Abraham, 1964. The Conduct of Inquiry: Methodology for Behavioral Science. San Francisco, Chandler Publishing.
- Lamb, Sydney M., 1966. Outline of Stratificational Grammar, Georgetown University Press, Washington, D. C.
- Martinet, André, 1960. Eléments de linguistique générale, Paris, Armand Colin.
- Mathiot, Madeleine, 1967. "The Place of the Dictionary in Linguistic Description," Language 43.703-24.
- Mathiot, Madeleine, 1973. "Quelques problèmes fondamentaux dans l'analyse du lexique," Meta 18.19-33 (Montréal).
- Mathiot, Madeleine, and Paul L. Garvin, 1975. The Functions of Language - A Sociocultural View, Anthropological Quarterly 48.148-56.
- Saussure, Ferdinand de, 1955 [1916]. Cours de linguistique générale, 5th ed. Paris, Payot.
- Trager, George L., 1963. Linguistics is Linguistics, Studies in Linguistics, Occasional Papers 10, Buffalo, N. Y.

TRANSMUTATION AS A DEFINING CHARACTERISTIC OF LANGUAGE

Hans R. Bergan
SUNY College at Potsdam

My point of departure has to do with the nature of man. I reject the rationalist contention that the mind of the newborn infant contains innate ideas as well as its linguistic correlate that it contains an innate universal grammar. Instead I uphold the empiricist position that what is innate is limited to capacities. But even though we are dealing only with capacities, I maintain that some of these are very special. They set man apart as being distinctive because they differ in kind and not just in degree from any capacities animals possess. What separates man and animal is not a gradation or a continuum but a quantum leap, and man is not just a more intelligent animal but a rational animal.

Indeed reason has traditionally been singled out as embodying man's uniqueness. Man has also been variously described as being able to think conceptually and symbolically. Finally, it has always been noted that what sets man apart is the gift of language. The common denominator underlying all of these distinctive traits is the capacity for abstraction. The essential element of this capacity is the ability to operate on a plane that is over and above and therefore independent from sense perception. While our sense perceptions continue to provide an input, the application of abstraction brings about a metamorphosis of these sense perceptions into new creations that exist wholly, and independently, on a mental plane. Here we must distinguish between abstraction and an operation I call generalization. Unlike abstraction, generalization is concrete and does not require an ability to operate on an independent mental plane. Generalization is therefore within the capacities of animals, whereas abstraction is an exclusively human ability. In other words, abstraction and generalization are distinctive in kind.

As the common denominator underlying man's uniqueness, distinctiveness based on the ability to abstract must be reflected in the structure of language. This means that language must contain certain features which are unlike any found in animal communication systems and which, because they are abstract, animals are incapable of mastering. What I am seeking is a specific feature in language that demonstrates abstraction, and I also want to find a means of isolating that feature experimentally. The feature I have in mind is one for which I suggest the name transmutation, while the means of identifying it is something I call the paradox principle. It should be noted that transmutation is not the same

as abstraction but refers only to a specific feature in language that exhibits the ability to abstract. Because it is the element that incorporates man's overall distinctiveness, I propose abstraction as the central defining characteristic of man. Similarly I propose its manifestation in language in the form of transmutation as the principal defining characteristic of language. Since the structure of language is more accessible to direct investigation than thoughts or concepts, transmutation - as one instance of abstraction - may clarify the nature of abstraction and may provide us with a means of pinpointing the distinctiveness that separates man from animal.

The Abstract Dimension in Culture

As a reflection of Man's ability to create an abstract dimension through his mind and to then project that dimension onto the physical world, culture has provided a favorite context in which to examine the distinctiveness of man. Before turning to language, let us therefore briefly review some of the points that have been made about man in his cultural setting.¹ One area in which man's abstract dimension comes to light is in his use of tools. There is a difference in kind between the primate's tool of occasion and man's conceptualized tool. Using a stick for digging or for spooning termites does indeed indicate an advanced degree of animal intelligence. However, it does not give evidence of any ability to abstract. A tool of occasion demonstrates abilities of association and generalization. We need not even insist that primates stumbled upon the use of such tools of occasion by accident. Instead we can grant them a certain ability to project into the future. There is no reason why a primate should not be able to anticipate the effect of hitting an opponent with a stick rather than with his body. After all many animals exhibit abilities of anticipation. Some animals are well known as perpetrators of novel ploys to deceive their prey or to foil the hunter. That requires anticipating the effectiveness of a stratagem.

It is amusing to note how, despite elaborate efforts to project a serious intent, the makers of the motion picture 2001 missed the point in their suggestion that subhumans turned into man with the discovery that bones can be put to effective use in striking an opponent. What these Hollywood subhumans did not accomplish was to invent the hammer or the battle ax. It is precisely the element of invention that sets the genuine tool apart from the tool of occasion. Invention requires not only anticipating a forthcoming effect, it requires abstract conceptualization. It is wholly in the mind that the genuine tool is fashioned and adapted to its purpose. It must be conceptualized and have a full-blown existence in the mind before it is manufactured into a physical object, and prior to its manufacture it has no equivalent outside the mind. The physical raw materials out of which the tool has been created are not equatable

with the tool but have to undergo a metamorphosis to become the tool. As a reality that does not exist out in nature but is created by the mind, the tool gives us an insight into the nature of abstraction.

Not only cultural artifacts, but cultural behavior too brings out this abstract dimension. Man's behavior surrounding holy water is unrelated to the physical characteristics of water. Sacredness is not contained within any physical object but is always projected onto the object by the mind. By contrast an animal relates directly to physical reality. While a dog may associate trees through an act of generalization with telephone poles and fire hydrants, he will always react to them as physical objects, whereas man is likely to react to trees through his system of cultural beliefs and conceptualize them as the residence of spirits. In the area of interaction, the gorilla may have a highly developed social system, but man differs from the gorilla in that his social system, being cultural, is not merely a system of physical association. Rather cultural interaction operates in terms of institutions and value systems that exist in the mind.

Presenting man as a naked ape driven by natural aggression and by the territorial imperative or the more recent fashion of turning man into a gene driven robot is a case of misplaced emphasis amounting to a misrepresentation of man. Man's natural drives remain, but as a residue, for they can be overridden and neutralized by his special capacity for abstraction. If man can be aggressive and territorial, he can also invent passive resistance. He can even overcome the instinct for self-preservation and elect martyrdom for the sake of an abstract principle.

While culture does provide useful insights into the abstract nature of man, it also illustrates the difficulty of pinpointing the transition from generalization to abstraction. Since man is capable of reacting to the same physical reality both directly and by abstracting it, just how and when does a tree change from a physical object into a concept? Or to put the matter in a more cultural perspective, how do we demonstrate the precise point at which physical associations become an institution or at which a reaction directly to a physical situation becomes a reaction to an idea? It is here that language may provide us with a useful tool.

Transmutation

Just as we have to distinguish between abstraction and generalization in culture, we must draw a line between transmutation and generalization, as the latter is manifested in language. Furthermore, if transmutation is to be a defining characteristic of language it should be a feature inherent in language and not just a model for the description of language. As Lamb (1974) points out with regard to transformations:

We must be careful to distinguish the phenomenon from the means of describing it....Many have supposed that transformations are the phenomena, rather than the means of describing the phenomena. Supporters of transformational description have often supposed that those who reject transformations are therefore failing to recognize the phenomena.

But even as a phenomenon of language transmutation nevertheless calls for a model of description that is capable of giving a clear account of the phenomenon, and not all models are suited to this task. Indeed, some models - notably those based on processes rather than on items and their arrangements (Hockett 1961) - are more effective in obscuring transmutation than in elucidating it. This need for an appropriate model means that the concept of transmutation can be applicable both to a phenomenon of language and to a means of describing that phenomenon. For example, the traditional dichotomy of etics and emics is basically a transmutational descriptive device. Similarly Lamb's realizations between strata approximate the concept of transmutation.

But even though certain models are more suited to its description, transmutation is nevertheless discernible across all linguistic models, a universality that reinforces the status of transmutation as a characteristic inherent in language. One reflection of this universality is that everyone who has attempted to analyze language has had to make some accommodation to the existence of levels. This applies even to transformational grammar, for not only does it contain separate components, but Chomsky also found himself obliged to revise his original theory by adding the levels of deep and surface structure. However, more important than the model itself is the fact that transmutation is discernible in the underlying linguistic data which a model seeks to account for. Being the least suited to the description of transmutation, transformational grammar provides a good illustration of this. More significant than the theoretical formulation are certain observations that Chomsky makes, such as the fact that it is quite remarkable how the native speaker is able to discern the similarity between the dissimilar John is easy to please and it is easy to please John, and the dissimilarity between the similar John is easy to please and John is eager to please.

Indeed, the key to transmutation lies in the ability to reconcile apparent contradictions between similarity and dissimilarity. It is this juxtaposition of similarities and dissimilarities I call the paradox principle. Whatever the model of analysis, whether it be deep structure or something else, the above sentences are not mere strings of items; but inherent in each sentence is a link to an underlying set of relationships. It is through this link to a set of relationships that exist over

and above and separately from the surface strings that we are able to reconcile the paradox. Transmutation lies in the interplay between two independent yet interdependent categories that in a model are best described in terms of levels or strata.

Transmutation operates not only in syntax. It is because different transmutations are involved that we can tell the difference between the same -er in finger, stronger, and singer. Each relates transmutationally to a different morphemic projection. Similarly, in the case of idioms we transmutationally reinterpret syntactic creations as lexical prefabs (Bolinger's term). In the area of phonology we have the well established distinction between phonetics and phonemics. Pike clearly had what I call transmutation in mind when he broadened the use of etics and emics to apply to a phenomenon in general. However, there are two reasons for proposing the separate concept of transmutation. Despite Pike's broad interpretation, there is a lack of unanimity on the use of etics and emics. In the minds of many the terms continue to be identified primarily with phonology and perhaps morphology rather than with a phenomenon of language in general. There are even those who would restrict the terms to relationships that are biunique. Secondly, if we examine the traditional use of the etics/emics dichotomy in phonology, we find that the definition of the phoneme gives rise to ambiguity and that at least one such definition is not transmutational. This is the widespread definition of the phoneme as a class of noncontrasting allophones.

We must be careful to make a distinction between a class, in the sense of a mathematical set, and a transmutational relationship, for which as far as I know there is no equivalent in mathematics. The error of tying etics and emics to set theory is tempting; for set theory is indeed applicable to such areas as the parts of speech, which are classes in the proper sense of the word. The class of count nouns, the class of transitive verbs, as well as the class of prefix morphemes are all sets. However, set theory is not applicable to etics and emics because an emic entity represents a unit and not a class. A linguistic unit is the product of a transmutation, whereas a linguistic class is the product of a generalization. We must therefore distinguish between a transmuted unit and a generalized class.

A generalized class is a set of members in which the whole is equal to the sum of its parts; or to put it another way, the nature of the set does not differ from the nature of its members. This is not the case in a transmutational relationship. A transmuted unit represents a relationship between parts and a whole in which the whole is not equal to but is something over and above the sum of its parts. Rather than representing a shorthand summation of its parts, a transmuted whole is a new creation that has an independent existence in its own right apart from, in spite of, and in addition to its parts. The parts do not make up the whole but rather serve as the input into its

creation. They are the raw materials that enter into the manufacture of the new whole. The transmuted whole resulting from such an act of creation is therefore a unit in itself, whereas a class represents only a summation of other units but is not itself a unit. Thus phoneme, morpheme, and word are units; consonant, affix, and noun are classes.

In order to emphasize transmutation as a phenomenon of language, let us turn to the universally acknowledged division between phonology and grammar, which Hockett calls dual patterning. It is safe to assume that the unanimity on this division is due less to a cooperative spirit among linguists than to a fact of language. The basis for the dual patterning dichotomy is that each level can be defined by the function it performs. Whereas grammar has a meaning conveying function, phonology is limited to a meaning differentiating function. Indeed, since it has no meaning conveying function, phonology stands out as a rather unusual feature to be included in a communication system, and it is indicative that only language has dual patterning.

It is on the basis of structural function that we can now make explicit the difference between transmutation versus generalization and between a unit as opposed to a class. A transmuted unit represents a whole over and above the sum of its parts because the function that defines the whole is not shared by the raw materials out of whose transmutation the whole has been created. Hence the morpheme is a linguistic unit distinct from its allomorphs, because the morpheme is a grammatical unit defined in terms of its meaning conveying function, whereas its allomorphs are phonological configurations that have a contrastive function. A generalized class on the other hand represents a whole equal to the sum of its parts because the class and its members share the same functional definition. If the prefix is defined as a morpheme that precedes a base then each individual morpheme that is a member of that class must occur in a position preceding a base. Similarly, if we define the count noun as a class that can be pluralized, then every count noun is subject to the same definition.

Transmutation therefore goes hand in hand with the functional definition of both the linguistic unit and the level or stratum on which it operates. This means that for a transmutational descriptive model the feature of levels does not suffice. Such a model must also provide a functional definition of each level in terms of the linguistic unit operating on that level. In this respect Lamb's stratificational approach does not provide a suitable transmutational model. Lamb has repeatedly stressed that, based on Hjelmslev, his is a purely relational model, in which units are inserted for the convenience of the analyst but lack theoretical status.

As a consequence of giving up the linguistic unit, Lamb's model also fails to adhere to a tight functional definition in establishing each stratum with the result that economy in account-

ing for relationships takes precedence over the structural function of units. Thus in dealing with the alternation between f and v in wife, wives Lamb will analyze in terms of an alternation between phonological components (distinctive features) what in a transmutational framework would have to be analyzed as an alternation between higher level units as wholes. Where Lamb on the basis of relational economy speaks of an alternation involving the contrastive feature of voicing, transmutationally - because we are dealing with a morphophonemic² (morphophemic) phenomenon - the alternation would be viewed as occurring between entire morphophonemic units. Handling the alternation on the level of phonological components amounts to an intermixing of transmutational relationships, because these components in terms of their function do not operate on the level where the alternation occurs.

The model I would propose as a more appropriate means of describing transmutation is the aspectual model as developed by the late Henry Lee Smith. Unlike Lamb's stratificational grammar, Smith's aspectual model is specifically transmutational in that it establishes separate levels on the basis of their structural function while also assigning full theoretical status to linguistic units.³

The Paradox Principle

Let me now demonstrate the difference between transmutation and generalization in terms of the paradox principle. Here we must distinguish between the single paradox principle as opposed to the double paradox principle. The single paradox states that differents can be sames; the double paradox principle goes a step further and states that differents can be sames while simultaneously sames can be different. The single paradox can be resolved by means of only a generalization, whereas resolving the double paradox calls for a transmutation.

The alternation of several phonological configurations as expressions of a single morphemic unit is a manifestation of the single paradox. Thus /gud/ and /bet/ in good and better and /s/ and /z/ in hats and songs are different with respect to phonology but are the same with respect to morphology. Confronted with the question whether the final elements in hats and songs are the same or different, one can reply either way and still be correct. The answer depends on whether one's parameter is phonology or morphology.

Stated in this manner the single paradox implies transmutation, and this is how human language users approach the occurrence of the single paradox in an etic/emic relationship. However, by itself the single paradox could be resolved by nothing more than a generalization, in which case it is not approached in terms of separate parameters such as phonology and morphology. As a generalization the equation of /gud/ and /bet/ or of /s/

and /z/ with a single function is no different than the dog's ability to equate trees, telephone poles, and fire hydrants with a single purpose or his ability to learn to equate chairs, tables, and sofas as being the same in terms of being off-limits for dog naps. Therefore the single paradox principle does not constitute proof that a transmutation has occurred.

However, in language the alternations between phonology and morphology also operate in the opposite direction, in which case they are referred to as ambiguity or homophony. Thus /left/ and /z/ are single phonological configurations that are ambiguous morphologically. Morphologically /left/ can be either the opposite of right or leave plus past tense, while /z/ can be either plural or third person as in songs vs. sings. However, since ambiguity is nothing more than a reverse manifestation of alternation, either one in isolation is an instance of the single paradox - which simply represents an or relationship - and hence can be handled by means of only a generalization. To use the example of the dog, the dog is doing the same thing when he generalizes several objects with one purpose as when he generalizes several purposes with one object.

Transmutation becomes differentiated from generalization only when we have to be able to perform the same / different operation in both directions simultaneously. In language we are faced with the task of being able to resolve the double paradox that /z/ is the same as /s/ but simultaneously differs from itself. In other words, we must be able to resolve the question how /z/ and /s/ in songs and hats are different in terms of phonology but the same in terms of morphology, while simultaneously /z/ and /z/ in songs and sings are the same in terms of phonology while differing in terms of morphology.

It is the double paradox that gives rise to the need for postulating separate levels in one's model as a means of accounting for the data. These levels are reflections of the mind's ability to operate in terms of separate categories, categories that do not derive from any sense perceptible input but that are established purely within the mind and are superimposed on the information that enters it. Herein lies the essence of abstraction.

It should be stressed that we have no grounds for knowing the precise shape of the abstract categories in the mind involved in transmutation and that our model does not constitute an indication that the mind actually processes transmutation in terms of levels or strata. However, what we can assume is that the capacity for establishing some kind of independent mental categories must exist in the human mind in order to enable man to keep the crosscurrents of information fed into the mind by the double paradox from jamming up the traffic. No such assumption is called for in the case of a generalization, and the ability to treat different phenomena in the same manner is a capacity demonstrated by even the most lowly organisms.

Stratification

The abstract nature of transmutation within language is further heightened by the fact that the input data on which transmutations operate, rather than being sense data, are for the most part themselves abstractions derived from previous transmutations. The transmutation of sense perceptible elements in language occurs only at the phonetic entry point into language. Here the sense perceptible phonetic input is transmuted into abstract phonological units on the basis of function. From that point onward the transmutational process is repeated to successively higher levels of abstraction. The etic input to these secondary transmutations consists of abstract units that are themselves the emic output of transmutations at the next lower level. The result is a layering of abstractions upon abstractions by the repeated application of transmutations.

This is the phenomenon of stratification. Whereas transmutation refers to the transition from one level to another, stratification designates the existence of a multiplicity of such levels within a structure. As in the case of transmutation, stratification - while referring specifically to a model or representation - can also be used as a term to designate the underlying phenomenon, so long as the two are not confused as being the same thing. The postulation of a layering of repeated levels of abstraction constitutes the model of stratification. The linguistic phenomenon lies in the potential for a multiple application of the double paradox principle in language.

Conclusion

I am proposing transmutation as the principal defining characteristic of language because more than any other feature it captures what makes language and the capacity for language different in kind and hence uniquely human. Transmutation allows us to make more explicit the capacities that differentiate man and animal. Both share the ability to segment reality, which is the capacity for making contrasts, and the ability to group segments into classes, the capacity to generalize. However, transmutation separates man from animal because it involves a new capacity that is revolutionary rather than evolutionary in nature. This is the ability to create categories that are abstract because the direct link to sense perceptible physical reality has been broken. If we add to this the capacity for stratification, we have an even more finely tuned instrument for evaluating the difference between man and animal.

Furthermore the double paradox principle provides us with a means of actually testing for the difference, something that has been lacking in the linguistic experimentation with primates which has become so popular of late. Missing in these experiments is a clear-cut idea of what to test for, namely a specific defining characteristic of language. This has led on the one

hand to the overall inconclusiveness of the results and on the other to extravagant claims, such as the following referring to the Gardners' sign language experiments with the chimp Washoe:

Since Washoe unmistakably combines and recombines signs to describe objects and situations new to her in perfectly appropriate ways, this wonderful experiment seems to have established beyond doubt that a chimpanzee is capable of learning true language (Russell 1971).

It should be immediately apparent at this point that the recombination of signs referred to involves nothing more than a capacity for generalization. This is true of the other animal experiments that have been done so far, including those involving the chimp Sarah, who progressed well beyond Washoe by mastering word order. While this is a remarkable feat, the mastery of tactic structure does not amount to a mastery of transmutation, much less of stratification.

When the chimp resolves the double paradox and does so in repeated layers, then he will have proven himself capable of abstraction and hence in possession of true language. At that point we will also be faced with the strong likelihood that the chimp is capable of thinking rationally in terms of abstract concepts. Meanwhile, the chimp and his fellow primates have a long way to go before they traverse the gulf that separates them from homo sapiens.

Footnotes

1. These points are worth repeating in the light of the shift in emphasis within the social sciences from man as distinctive in kind to man as a more intelligent ant.
2. For further details on the morphophone see Bergan 1976, Hoffman 1975, Smith 1967 and 1968.
3. Perhaps my use of the term unit to refer to the phoneme or the morpheme is synonymous with level. There is a class relationship between the phoneme and the inventory of phonemes of a particular language, and the term unit applies perhaps more appropriately to the latter. The important thing to note however is that the class relationship does not apply to the connection between a phoneme and its allophones or a morpheme and its allomorphs.

References

- Bergan, Hans R. 1976. "The Morphophone: A Reassessment of Phonology." In The Third LACUS Forum, edited by Robert J. Di Pietro and Edward L. Blansitt, Jr., 580-593. Columbia, S. C.: Hornbeam Press.
- Hockett, Charles F. 1961. "Linguistic Elements and Their Relations." Language 37:29-53.
- Hoffman, Melvin J. 1975. "Problems from Implicit Premises in Classical Phonemics: Examination, Identification, and Current Implications." In The Second LACUS Forum, edited by Peter A. Reich, 129-140. Columbia, S.C.: Hornbeam Press.
- Lamb, Sydney M. 1974. "Mutations and Relations." In The First LACUS Forum, edited by Adam Makkai and Valerie Becker Makkai, 540-557. Columbia, S.C.: Hornbeam Press.
- Russell, Claire and W.M.S. Russell. 1971. "Language and Animal Signals." Reprinted in Language: Introductory Readings, edited by Virginia B. Clark, Paul A. Escholz, and Alfred F. Rosa. 1972. New York: St. Martin's Press.
- Smith, Henry Lee, Jr. 1967. "The Concept of the Morphophone." Language 43:306-341.
- _____. 1968. English Morphophonics: Implications for the Teaching of Literacy. Oneonta, N.Y.: New York State English Council.

LINGUISTICS IN MEDIAS RES

Joe Darwin Palmer
Centre for Teaching English
as a Second Language
Concordia University
Montréal, Québec

"The practical work of describing languages goes forward -- to make dictionaries, assimilate minorities, provide bilingual instruction, train translators -- with or without help from linguistic theory."

Dwight Bolinger, Aspects of Language

"Some people have a way with words; other people not have way."

Mary Hartman! Mary Hartman!

Just as Voltaire is said to have defined philology as "la science où les voyelles ne comptent pour rien, et les consonnes pour peu de chose", recent attempts to define applied linguistics have become similarly formal and abstract to the point of airy-fairness.

Concern about the definition of applied linguistics has become a remarkable feature of professional gatherings. The Stuttgart AILA Congress in 1975 sponsored a round table discussion on this topic. The Applied Linguistics Special Interest Group of TESOL did the same in Miami Beach in 1977. Graduate students are often at a loss to say just what the discipline consists in and of. In this paper I shall attempt to relate to each other some of the gleanings from this haphazardly harvested field, and to suggest some ways of living with the metalinguistic paradox.

The currently most popular metaphor for explaining how linguistics has got itself mixed up with all sorts of endeavours, from mental health to docimology, leans on S. Pit Corder's definition of applied linguistics in the Edinburgh Papers . . . :

a set of related activities or techniques mediating between the various theoretical accounts of human language on the one hand and the practical activities of language teaching on the other (1975).

This is a straight-forward and limited definition. Here the term mediate suggests a sort of priestly function for the applied linguist -- not between man and God, of course, but between linguistics and most intellectual activities -- intermediation,

mediation, medium, go-between, match-maker, promoter, entrepreneur, the wire that carries the current, direct or alternating.

The mediation jargon is used by Edward Anthony (1977), Russell Campbell (1977), and most pointedly in Henry Widdowson (1975). Following Widdowson's apologia for stylistics as the mediator between linguistics and literature, and between language and literary criticism, I here present what I trust to be a faithful paraphrase of the main argument:

Linguistics is the thoughtful study of language. It takes in many subjects: language families, field methods, philology, phonology, physiology, theories about how languages work and how people learn them, etc.

Linguistics is a discipline -- a set of procedures and ways of thinking and knowing that provide the axioms upon which good theories are based. Disciplines like linguistics are defined in terms of the theories that make their descriptions accurate and their predictions true.

Subjects, on the other hand, are the components of disciplines. Subjects may consist of knowledge drawn from two or more disciplines. The comprehensiveness of subjects depends on the educational levels at which they are studied.

As a way of knowledge, linguistics can be used to give intellectual substance to many subjects. These subjects in turn may be viewed as components of other disciplines. When linguistic subjects are used for the purpose of relating knowledge of language to knowledge of other disciplines, we can say that the linguistic subjects mediate between two disciplines. For example, the subject dialectology mediates between linguistics and sociology. The subject stylistics mediates between linguistics and literary criticism.

Several subjects may mediate between linguistics and one other discipline. For example, grammar, language policy, semantics, phonology, phonetics, ethnology, field methods and bilingualism, in addition to dialectology, may mediate between linguistics and sociology. Furthermore, one subject may mediate between linguistics and several other disciplines. For example, grammar mediates between linguistics and most, if not all, of the other disciplines listed below.

	<u>SUBJECTS</u>	<u>OTHER DISCIPLINES</u>
THE DISCIPLINE OF LINGUISTICS	{ - acquisition - bilingualism - computation - contrastive analysis - curriculum design - dialectology - error analysis - ethnology - field methods - grammar - language history - language policy - language teaching - methodology - phonology - lexicology - semantics - speech - stylistics - English, Spanish, etc.) . . .	{ - anthropology - archeology - cybernetics - ethnography - lexicography - literary criticism - pathology - pedagogy - physics - political science - psychology - sociology - statistics - testing - translation . . .

Applied Linguistics is, then, the use of linguistic subjects whose contents may improve practical work in those disciplines that include the use of language.

One must note the assumption made here that linguistics is something other than phonology, grammar, language history, philology, lexicology, semantics, stylistics, error analysis, and so forth. That this assumption is false is clearly evident in the failure to define linguistics as consisting of anything other than these subjects.

Surely it is clear that linguistics does not have any independent existence outside the subjects that comprise it. A discipline is a constantly changing colligation of subjects, a more-or-less traditional branch of learning that has its own self-validating procedures, one that depends upon particular methods and approaches that change as the intellectual climate changes. Pure or theoretical work in a discipline cannot exist without practical and applied uses of the knowledge that makes up the discipline. The formulation of theories depends upon the practical experience gained from trying to apply ideas to real situations. To quote John Oller:

Applied linguistics needs theoretical linguistics and conversely, the theorists need to test their theories by applications, and both require empirical research techniques in order to accomplish either objective. (1977)

The name applied linguistics is a tautology. The metaphor that relegates applied linguistics to an intermediate position merely increases category clutter; entities should not be multiplied unnecessarily. Shades of thesis - antithesis - synthesis! Word magic. Should we call the art of cooking applied chemistry? Is a Heinz (57 Varieties) not a dog?

Edward Anthony (1977) claims that applied linguistics is not an antonym of linguistics; applied linguistics in isolation is linguistics. Here Anthony implies that applied linguistics is not a thing, not a reified construct, but a process of relation.

Bernard Spolsky (1977), echoing a second popular way of defining applied linguistics, says that applied linguistics is problem-oriented, and it finds solutions in many disciplines. Russell Campbell (1977) states that applied linguistics is "a problem solving enterprise". The problem-solution nexus certainly lends itself to an operational definition, a definition much less distastefully related to two-valued thinking. In this manner of speaking, Peter Strevens (1977) says that applied linguistics is "a multidisciplinary approach to the solutions of language-related problems". In 1975, he wrote that "only applied linguistics responds in a principled way to any language-related problem".

A more fanciful, and consequently more perspicuous view of applied linguistics was stated by Robert Kaplan (1975). He posited a comparison of applied linguistics and theatre. In theatre various arts are combined -- music, literature, acting, set design, the plastic arts, dance. In applied linguistics we find "the point at which all study of language comes together and becomes actualized". This is not a definition at all, but a description by way of simile, and to my mind the most truthful of the three figures of speech that are now being used to say what applied linguistics is: the mediation metaphor, the problem-solving analogy and the theatre simile. Kaplan's simile reminds one of S. Pit Corder's (1975) statement that "applied linguistics is everything that was discussed in its name by the 1975 gathering of world specialist in Stuttgart" (See the AILA Bulletin No. 1(20), 1977, for examples), which definition Strevens (1975) excuses as a working rather than as a fundamental definition much like the zoologists' definition of a dog as a "four-footed mammal recognized as a dog by another dog". Moreover, such a working definition is given quite seriously by S. Pit Corder (1977) when he says of applied linguistics that "We who do it know what it is."

Whatever it is, it got its name in the United States during the 1940's from some persons who wanted to be identified as scientists rather than as humanists, according to William F. Mackey (1966). The name was not quickly adopted in the United Kingdom. Stevens (1977) reports that upon asking for an academic appointment in London as a professor of applied linguistics, he was given an appointment as a professor of contemporary English, for the committee in charge did not know what applied linguistics was. However, in the United States, where novelty is precious, and reverence for science the stuff of popular myth, applied linguistics as applied science was soon welcomed into academia. Assuming that linguistics can be applied science, a generation of scholars produced language-teaching materials based upon atomistic descriptions of tiny pieces of languages, enjoyed official and popular support, asserted their independence from departments of literature and language teaching, wrote graffiti on old icons, and smugly proclaimed that linguistics, as a natural science, would cure language ills.

And still they continue to force pieces of languages into Procrustean beds. As Oller says (1977), "now people seem to be trying to distinguish the real applied linguists from the linguists who are posing as such". I think that Oller means that linguists, mad with theory, are seen to be posing as messiahs of new orders that have the guise of technology -- as if "scientific" linguistics can be applied directly without radical application of knowledge from other disciplines. Because of the exigencies of the academic market place, some linguists pander to educational and social needs. Such opportunism is not the applied linguistics that we are trying to get at.

Tony Robson (1977) pleads that training in applied linguistics is too narrow, that the name itself should not be limited by definition, that students of applied linguistics should be sent to traditional departments to learn about standardization and social development and translation -- subjects that are substantial and useful and of this world.

Let us be wary of pretending too much that linguistics will provide a language-independent explanation of how language works. Linguistics is not a natural science. We may say that language is metaphysical, for the interesting, formal part of language does not belong to the world of nature, but resides in our understanding. Linguistics as the study of language treats of the relations obtaining between the underlying reality and its manifestations. B.L. Whorf claimed that "each language . . . conceals a unique metaphysic (Webster III)". That is why modern philosophy, particularly Ordinary Language Philosophy, having thrown out pre-Kantian metaphysics as unscientific, soon turned to the uses of language for its inspiration. Compare Wittgenstein's "we must plow over the whole of language (Ian Robinson, 1975)".

Thus linguistics not being a natural science, applied linguistics is not an applied science having the technological benefits and spin-offs that we have learned to expect from research and development.

It is hard to get at applied linguistics. Perhaps the name is simply inappropriate, having many connotations that breed unfortunate assumptions. Kaplan (1975) called applied linguistics a "fuzzy field". Thomas Buckingham (1977) writes that "the name applied linguistics is misleading and inadequate, both too narrow and too broad . . . an ill-chosen name". Douglas Brown (1977) says that "like linguine, applied linguistics sticks to the roof of the mouth".

Since, as Oller (1977) claims, "applied linguistics is at the frontiers of ignorance", should we not discard the name?

Let's call it language teaching, or stylistics, or dialectology, or phonology, or syntactic theory, or whatever it is.

References

- A.I.L.A. Bulletin, No. 1(20)
May be obtained from Prof. A. Zampolli, 36 Via Maria,
56100 Pisa, Italy
- Corder, S.P. "Applied Linguistics and Language Teaching" in
Allen, J.P.B. and S. Pit Corder, eds. Papers in applied
linguistics, pp. 4 - 5. Oxford 1975.
- Mackey, W.F. "Applied Linguistics: Its Meaning and Use", English
language teaching, 20, 3, pp. 197 - 206, May 1966.
- Robinson, I., The new grammarian's funeral: a critique of
Noam Chomsky's linguistics, Cambridge, 1975.
- Widdowson, H.G., Stylistics and the teaching of literature,
Longman, 1975.
- Wittgenstein, L., Remarks on Frazer's Golden Bough, p. 34,
quoted in Robinson 1975.
- Statements attributed to Brown (Michigan), Corder on knowing what
it is (Edinburgh), Robson (Center for Applied Linguistics),
Spolsky (New Mexico), and Strevens on language problems and
an academic appointment (sometime Essex) are from the TESOL
round table 1977.

Kaplan 1975 and Stevens 1975 refer to papers prepared for the 1975 AILA Congress Round Table and reprinted in Toward a redefinition, below.

Statements attributed to Anthony (Pittsburgh), Buckingham (Houston), Campbell (UCLA), Kaplan (USC), Oller (New Mexico), and Stevens appeared in Toward a redefinition, below.

Buckingham, I., ed., Toward a redefinition of applied linguistics: current statements on the nature and functions of applied linguistics with particular emphasis on its context in language learning and teaching, Applied Linguistics Special Interest Group round table, TESOL Convention, Miami Beach, 1977, mimeographed.

TWO FORMS OF REDUCTIONISM
Charles Ruhl
Old Dominion University

During the twenty years of Transformational Grammar, one of its most persistent critics has been Dwight Bolinger. Bolinger's forte is an emphasis on detail and complexity, a disdain for easy and premature generalization; and his objections usually start from specific points, as his criticism of Postal's (1970) analysis of the verb remind (Bolinger 1971). But his criticisms are fundamental too, especially his response to Katz and Fodor's (1963) proposal for a semantic theory (Bolinger 1965), and his recent article "Meaning and Memory," in the first issue of the journal Forum Linguisticum (Bolinger 1976). I want to examine this last article at length, not only because it reveals a much-needed sensitivity to the richness and diversity of language, but also because its deficiencies short-circuit Bolinger's own appreciation of his ideas and lessen the article's effectiveness in promoting the linguistic attitudes he desires.¹

As his title suggests, Bolinger's subject is memory. Through our preoccupation with Theory, he says, we have overestimated the role of generativity and underestimated that of memory.

- (A) Specifically what I want to challenge is the prevailing reductionism -- the analysis of syntax and phonology into determinate rules, of words into determinate morphemes, and of meanings into determinate features. I want to take an idiomatic rather than an analytic view, and argue that our language does not expect us to build everything starting with lumber, nails, and blueprint, but provides us with an incredibly large number of prefabs, which have the magical property of persisting even when we knock some of them apart and put them together in unpredictable ways.² (p. 1)

These prefabs must be memorized; often their meanings cannot be derived from the sum of their parts. This creates a much more heterogeneous language. Bolinger sees the present homogeneous theories as reductionistic, because they oversimplify the complexity of linguistic knowledge.

- (B) Formal deductive grammar since its beginnings has striven to eliminate variety, not only in the idealization of the system away from the realities of 'performance' but in the inner structure as well. This of course is the basis of reductionism. If there is the slightest possibility of accounting for something in terms of elements at a lower level, that possibility is adopted, often in the teeth of two

kinds of negative evidence: first, that the procedure may give inaccurate results, second that it may be psychologically unnecessary. All the debate up to the present has been on the first point, that of accuracy. But now it is time to take account of the psychological side and give the greater weight to Antilla's [(1972,349)] remark that 'even the most "obvious" cases can be stored separately' -- which is to say that even when a linguist CAN analyze, it does not necessarily follow that he ought to. If grammars merely aimed at descriptive adequacy, this argument could not be advanced; but when grammarians begin to claim psychological reality for their models, they are presuming on explanatory adequacy of a deeper sort than they bargained for. (p. 12)

Bolinger's point can be amplified by reviewing some of his criticism of Katz and Fodor (Bolinger 1965, 446-448). He notes that their theory, an extension of TG, assumes a relatively small number of minimal, discrete and absolute units, treated as if they were learned totally and at once. He observes that the requirements look suspiciously like those needed for a computer, not a human being. In contrast, Bolinger believes that meanings and syntactic patterns are gradient as well as discrete; that meaning is not fixed absolutely, but is modulated by context, including extralinguistic context; that meanings carry a "something like" metaphoric extendability; and that associations of form and meaning are learned gradually, continuously, with both under- and overextensions, and learned in contexts, in collocations. Such a view of language expects that, of necessity, many constructions will remain in the mind as wholes, even after analyzed into parts, and that the wholes will carry meanings that are not merely the sum of their parts.

Although Bolinger does not describe it as such, the difference of views is that between an atomistic-mechanistic view and an ecological-holistic view. In Ruhl (1977) I noted that the mechanistic view seems to be the favored mode of operation of the conscious mind; it is an isolating, idealizing, constructionist view. To account for language, mechanist theories deal with only a few factors and assume that generalizations are relatively few and wide-ranging; that is why Bolinger calls them homogeneous views. In contrast, ecological theories anticipate many factors, many of them extralinguistic, which produce many more generalizations of much more limited scope; that is why Bolinger calls them heterogeneous. In holistic orders, parts develop in the context of wholes; the wholes account for the parts, not vice versa. As the ecological crisis develops in this century, we see the severe limitations of idealizing-mechanistic procedures. Taking a deductive approach to language is akin to taking efficiency as the most laudable virtue, and it has the same dehumanizing results. We have only begun to appreciate the great complexity of holistic orders, and it is the great strength of Bolinger's work that he

has persistently and in great detail shown us the dimensions of this complexity, revealing factors such as gradience, syntactic blends, inference, stereotyping, and others that are too inconvenient for our impatient theories.

Bolinger explains clearly the implications of this complexity for linguistic research:

- (C) Accepting a principle of heterogeneity means a different attitude, I think, toward the object of our study. Prime attention to the overarching system and more or less incidental attention to the local detail follows naturally from the assumption that the grammar is homogeneous and self-confirming. In a formal deductive system everything is explained when everything fits. There is no need to worry about missing a detail or two because sooner or later any mistake or oversight will show up in a grinding of the gears. It even seems rather petty to insist on full and precise documentation in each separate province of the realm. The power of the system as a whole to reveal a flaw is so great that mastering the grand design outweighs any small help that can be picked up from one of its mere neighborhoods. But once homogeneity is denied, this security is gone. Separate -- and, yes, ad hoc -- explanations for each part become essential. It is no longer safe to assume that the system will correct itself. Unquestionably it will in many ways; certainly there are local regularities as well as universal ones; but the self-corrective remedies must be sought AFTER one has canvassed as thoroughly as possible in each neighborhood, approaching the indwellers on their own terms. (pp. 12-13)

But at the heart of Bolinger's argument is a contradiction, one that is readily apparent when we see that in his specific examples he does not investigate as "thoroughly as possible" as he advocates in (C). The requirements he sets for linguistic research are so demanding, so contrary to our usual attitudes, that even he deserts them. From a holistic perspective, we should say that TG, in spite of its increasing complexity, is still essentially oversimplified; language is much more complex and much more ordered than even TG assumes. But Bolinger veers in the opposite direction. He describes his view, in (A), as an idiomatic view. At first glance, idioms may seem to be the ideal evidence to use against TG. The TG vision of order has been shown incorrect in its particulars so often that it is natural to react with the claim that language is highly disordered. Idioms have been used to demonstrate this disorder. Yet an argument against TG using idioms has very little force. Transformationalists readily admit that there are idioms, define the term similarly, and usually agree with the critics about which constructions should be so designated. This is not surprising: as presently defined, the term is a mechanistic term; and far from posing a

threat to mechanistic theories, it can be a useful tool in warding off negative data.

The term "idiom" in linguistics is used to describe an extremely crude and uncritical collection of characteristics that are tacitly taken to be implicationally linked. If a construction shows one characteristic, it is automatically assumed to possess the others. The usual explicit definition, with varying degrees of sophistication, is as Bolinger states it: constructions that "have meanings that cannot be predicted from the meanings of the parts." (p. 5) I criticized this definition in Ruhl (1977), showing that generalizations are lost if the words in so-defined idioms cannot be related to the same words elsewhere. But the implicit definitions are more crucial in Bolinger's article. He assumes that a construction is an idiom when it is a set phrase or syntactically frozen, even when its meaning can be predicted from its parts. His line of tacit justification for this seems to run as follows: If a phrase is frozen it can not be generated, but must be stored whole. If it is stored as a unit, then its words cannot be linked to the same words elsewhere. Thus, the construction is an idiom.

As an example, consider his discussion of out of patience:

- (D) Suppose we took the phrase out of patience and looked for an underlying representation. It would have to include the same out of that is found in out of money, out of time, out of ice cream, out of anything that one formerly had a supply of but has no longer. It never dawned on me, until the 24th day of January 1973, that the out of in out of patience has any connection with the other out of -- penetrating the source of the expression was one of those odd strokes of illumination that we get every so often with something we have been using all our lives. It is clear enough that most people do not associate the two, because if they did they would not turn out of patience into a command, or use intensifiers with it (Don't be so out of patience! *Don't be so out of money!) When we say out of patience we are not pulling out of and patience separately from storage and putting them together but retrieving the whole thing at once. (p. 3)

Bolinger gives two arguments. Consider first his discovery of January 24, 1973. In a holistic view, most of our linguistic knowledge is unconscious. Our conscious linguistic knowledge may be quite different in some respects, for at least four reasons: it is limited by the selective nature of the conscious mind; it has lost touch with the basic organization schemes developed in childhood; it is unduly influenced by social proprieties and theoretical commitments; and it must be ordered to deal effectively with external realities that the unconscious ignores. Considering that Bolinger is defending the great storage capacity of memory, it is surprising that he accepts the tacit assumption of transformationalists that elements are stored only once. Coupled with

this assumption is his complete faith in introspection; in assuming that his conscious awareness is an accurate and complete guide to his linguistic knowledge, he follows mechanists in their over-estimation of conscious abilities.

Consider now Bolinger's argument using intensifiers and commands. If language is ordered holistically, the number of factors governing any construction can be quite numerous, and it is hard to tell from a small amount of data what all the factors and their interactions are. The mechanist, assuming a relatively simple order (and one that is more apparent), will habitually rush to a conclusion on the basis of a few selected examples. The conclusion will be either a simplification or a judgment that no order exists. Bolinger here, ignoring his advice in (C), behaves like a mechanist. In his criticism of Postal, he noted that syntactic and semantic effects can easily be attributed to the wrong source, but he does not consider whether the difference in acceptability of the intensifiers and commands may be caused by the semantic difference of money and patience. Yet, we also have Don't be so impatient versus *Don't be so penniless.

There are also related constructions:

- (1) I'm (completely) out of patience (with him).
- (2) I've (completely) run out of patience (with him).
- (3) My patience (with him) has (completely) run out.
- (4) I've (completely) lost patience (with him).
- (5) I don't have any patience (with him).

The pattern similarities are striking. With him occurs after patience in each sentence, even when it is separated from out of. The out of out of seems to be the same as the out occurring after run. Completely occurs in all but the last sentence, and all can occur in its place in (1) and (3). It seems unlikely that the words of out of patience would not be identified by a speaker with other uses of out and patience.

Note also that out of money partially parallels out of patience:

- (6) I'm (completely) out of money.
- (7) I've (completely) run out of money.
- (8) My money has (completely) run out.
- (9) *I've (completely) lost money.
- (10) I don't have any money.

One discrepancy is (4) versus (9); my, only optional in (4), is necessary in (9). In correspondence, Bolinger notes another discrepancy:

- (11) We're out of money. -- When did you run out?
- (12) I'm out of patience. -- *When did you run out?

Yet, these discrepancies may also be attributable to the difference between money and patience. Whether or not the second part of (12) is as bad as Bolinger thinks, there is a comparable oddity in the second part of (14):

- (13) I have money. -- When did you get it?
- (14) I have patience. -- *When did you get it?

We must also note other possibilities for money and patience, such

as the fact that patience also occurs with wear out:

(15) "My goddam patience is wearing out." (Bernard Malamud)

In making any comparison we must consider both the similarities and the differences. It is certainly a mistake to make hasty generalizations: to assume, for example, on the basis of the parallel of (1-3) and (6-8) that money and patience will consistently pattern the same. Hasty generalizations of this sort have often been made, under the mistaken belief that principles of order range over wide areas of the grammar. But it is also a mistake to assume that several discrepancies of pattern imply that there are no generalizations at all, or that speakers will not make them. We can extend this point by looking again at Anttila's remark (in (B)). Although it is rash to assume that if a connection can be made it will be made, it is equally rash to assume that if a connection need not be made then it won't be made. Until we have a better understanding of the human mind, and until we investigate the data of the language in more detail than has been our custom, we have little justification for either position.

Bolinger may be right that out of patience is stored as a phrase. This does not mean that it is an idiom; it may indicate additional principles of order. As I noted in our correspondence, a speaker may store out of bread, out of milk, and out of beer as units also. Phrases used often are automatically memorized. Phrases that are similar in structure, and so generalized in the mind, may also be treated quite differently by the mind if they differ in utility. But Bolinger notes further evidence which suggests that out of patience is a special case; he argues (in correspondence) that while we can conjoin to get out of bread and milk, patience cannot conjoin with anything. He also notes that other abstract words like forgiveness, strength, and endurance cannot substitute for patience. But, for the "supply" sense of out of that we are considering, I have no difficulty substituting strength, endurance, energy, and feeling. I also see nothing wrong with out of time and patience, out of strength and patience, out of patience and energy, and others. But I have found no examples of these to substantiate my intuitions, so I will grant the point that abstract objects and conjunctions are limited. Again, however, there may be systematic reasons for the limitations. Until we have a complete analysis of abstract terms, especially those designating human attributes such as patience, forgiveness, strength, and endurance, we do not know if we are justified in treating them as paradigmatic equals for the syntactic tests Bolinger uses. We do not have an ordered analysis of the construction out of, and thus do not know how its uses relate to each other. Constructions like out of sympathy/pity/laziness/gratitude/a feeling of remorse do not have the "supply" sense but a "motive" sense. Bolinger has often noted the tendency of syntactic constructions to stereotype: in a potentially ambiguous construction, one sense is favored and others are marginal or impossible. It is possible to use out of sympathy in a supply sense, or out of patience in a motive sense, but speakers may not exploit these possibilities because the predominant senses are

more useful. Similar considerations may apply also to the difficulties in conjoining.

It is obvious that such speculations do not prove my point. I offer them only to show that Bolinger's conclusions are premature and conditioned more by preconceptions than by an adequate survey of the relevant data. It is often the case that the dominant view in a field so fixes the terms of debate that critics are easily compromised into unsatisfactory assumptions. Bolinger seems to have accepted without question the tacit dichotomy (which even transformationalists may not support explicitly) that a construction is either generated by rules, and thereby related to the rest of the grammar, or else is an idiom, unrelatable to other elements. But the mind is not necessarily limited to our present conceptions of generativity; it may be able to make connections and distinctions that are hard to define with present deductive methods. If out of patience is a set phrase, we will have to list it as such. (Although, if it is a set phrase simply because it is highly useful, transformationalists may want to assign it to performance.) But in light of other related expressions, we must also specify the generalization.

We must also resist arguments based on simplicity. The restrictions on out of patience may be such that the simplest grammar would simply make a general ban and list out of patience as an idiom. But Bolinger's point about memory implies that the mind need not operate under stricture of simplicity, but may have considerable redundancy. It is also possible that different speakers handle matters differently, and it may be difficult on the basis of everyday use to distinguish them; however, people who are highly fluent or creative may owe these talents to superior mental organization. If our grammars aim for psychological reality, they must reflect the full range of possibility. In any event, if there is a conflict between data and model in any of these issues, the model must give ground. Calling out of patience an idiom is a clear sign that preconceptions are too limiting. Bolinger here resembles some transformationalists in that the conclusion he desires is so narrowly pursued that little of the necessary data is considered. We see this even more vividly in the next example.

Bolinger's views on gradience lead him to conclude -- correctly, I think -- that there is no sharp break between morphology and syntax, and that within both there are varying degrees of adhesion of parts. This conclusion follows from a holistic view, in which many principles of order create many multi-valued possibilities. But Bolinger sees his claim as a support for a lack of order and a need for more constructions to be memorized as set phrases. He wants a word that will function somewhat like an affix and chooses ago:

- (E) ... take the word ago. It has several interesting characteristics. First, it is always suffixed -- we say a year ago, not *an ago year; and we do not use it independently -- there is no *He got there ago meaning 'he got there at some time past.' Second, it

is always suffixed to expressions of a particular kind, namely those referring to time. In this respect it differs from its synonym back: either ten years back or ten miles back is normal, but not *ten miles ago (unless we intend to metaphorize the noun as a noun of time: a grief ago). Third, it is unstressed: It happened a year back but It happened a year ago. With definite temporal quantities there is almost complete freedom, but the indefinites carry some peculiar restrictions. Long ago is fine, but not *short ago, although either a long time ago or a short time ago is normal. As for much and little, they are barred completely, even with the word time added: *much ago, *much time ago, *little time ago, and so on. The restriction is not because much and little refer to quantity whereas long refers to extent, because long cannot be replaced by other adjectives of extent either: a long time ago but not *an extended time ago. With indefinite plurals, centuries ago is all right but ?decades ago and ?eons ago are doubtful and *millenia ago I think is impossible. So ago behaves in all important respects like an unusually productive affix. The only real difference I can detect is the word's high degree of semantic integrity. Ago stays the same in meaning. Few if any lexical suffixes in English do (though there may be some prefixes that do).... The question is, why do we not generate *an extended time ago if we generate a lifetime ago...? It is not because the generative mechanism is lacking. I SUGGEST THAT AT LEAST IN PART WE DO NOT DO IT BECAUSE WE HAVE NOT HEARD IT DONE. WE HAVE NO MEMORY OF IT. (pp. 3-4)

It is more obvious in this case than with out of patience that some of the irregularities Bolinger notes are created by other factors. First, long ago is all right but not *short ago because long can occur with an implicit sense of time while short cannot:

(16) We won't have to wait (a) long (time).

(17) How long (a time) will we have to wait?

Second, since Bolinger compares ago with back, he should have noted the parallel strangeness of decades / eons / millenia back. Note also the strangeness of millenia earlier, millenia later, and millenia from now. These three words are more marginal in use than other time words. As Bolinger notes, if we use a word in a construction often, it makes it easier to use later. A speaker may have rules in his grammar that allow certain possibilities, but since the occasion for their use rarely arises, he may not use them and may even find them strange when someone else uses them. As Bolinger has often noted, an impression of strangeness may also be created by citing a construction in isolation; in the following eons ago does not seem odd:

- (18) ...the record shows that astronomers hold to a peculiar dogma akin to the biblical story of Creation, that the solar system has remained unchanged since it was created eons ago. (Livio C. Stecchini)

Third, I do not find an extended time ago as strange as Bolinger does, but in any event an extended time back/earlier/later/from now are equally strange.

Bolinger is correct when he says that ago requires a time noun (implicit in long ago). But his discussion of much and little overlooks the obvious: ago requires a COUNT noun phrase (also implicit in long ago: see (16-17)). Thus, we can have many years ago and a few years ago. Again, Bolinger does not consider whether any limitations on indefinites may come from the indefinites themselves. There are more indefinite possibilities than he notes, as follows:

- | | | |
|------|-----------------------|----------------------|
| (19) | a while ago | ?a time ago |
| | a short while ago | a short time ago |
| | a little while ago | *a little time ago |
| | a long while ago | a long time ago |
| | a good while ago | *a good time ago |
| | a good long while ago | a good long time ago |

A significant irregularity is not *little time ago, but *a little time ago; yet, as the paradigm indicates, this is likely a limit on time. We also have some in various constructions:

- (20) ...the quietly starved brightness of my senses which some while ago made the transition past any need for sleep... (James Agee)
- (21) The argument started some time ago... (Durham Morning Herald)
- (22) He had already noticed that Migdone was a little more drawn about the mouth since he saw him some days ago... (Jesse Bier)
- (23) It was Larrabee who presented Velikovsky's theories to the world, some thirteen years ago. (Donald H. Menzel)

The role of ago as a suffix is thrown into doubt by the possibility of two years or so ago and similar constructions.

The comparison of back with ago illustrates a chronic weakness of Bolinger's work: an equivocation on the term "synonym." This term is as much misused as "idiom" and is as poorly defined. When Bolinger first states that two words are synonymous, the only way in which he can be correct is by the definition "conveys virtually the same meaning in a particular context." But he then submits his so-designated words to syntactic tests that require a more stringent definition: "exactly the same in meaning." A good illustration of this occurs elsewhere in the article when he is trying to make the point that some selectional restrictions are not governed by semantic regularities but are simply idiosyncratic. He notes:

- (F) ...the verbs to look at and to regard are synonyms, and we can say either He looked at me strangely or He regarded me strangely. But whereas we can say

simply He looked at me and let it go at that, or say He looked at me for five minutes, we cannot say either *He regarded me or *He regarded me for five minutes. The verb to regard in this sense has to collocate -- or colligate -- with a manner adverb. (p. 8)

But the data he cites shows that look at and regard are not synonymous, except in the loose sense that his first pair of sentences mean approximately the same thing. This was precisely his criticism of Postal: Harry reminds me of Fred and Harry strikes me as being similar to Fred are roughly interchangeable as messages, but only because the differences between strike as similar and remind are either unimportant or neutralized. It seems ironic that the person who has been most perceptive in showing the non-synonymy of transformationally related sentences would have such a loose sense of synonymy. The supposed synonymy of back and ago is even more dubious. As Bolinger observes, the first is not limited to time, so making them synonyms is like making parent and mother synonyms. But the difference between the two, as I will now show, is greater than Bolinger notes.

We can account for the other characteristics of ago that Bolinger cites by defining its basic characteristic: it is a deictic word. Consider these sentences:

- (24) John said that he saw the movie two weeks back.
- (25) Last year John said that he saw the movie two weeks back.
- (26) *John said that he saw the movie two weeks ago.
- (27) Last year John said that he saw the movie two weeks ago.

Consider the interpretations in which the final time expression indicates when John saw the movie. In (24), with back, we can take either John's view or the speaker's view; the two weeks can be earlier than when John spoke or earlier than when (24) was uttered. In (25) only John's view is possible, since last year makes the speaker's view impossible. In (26), with ago, we are required to take the speaker's view, because ago is deictic. Thus, (27), which is parallel to (25), is semantically ill-formed, since the speaker's view cannot be taken.³

Note that by considering the possibility of ago occurring before the noun, Bolinger assumed that it was an adjective; the prohibition is regular if it is an adverb. But given the deictic status of ago, there is an explanation for the syntactic limitation even if it is an adjective. In his criticisms of the transformational derivation of attributive adjectives from relative clauses (1952; 1967), Bolinger notes that occurrences before the noun often result in the adjective being taken as characteristic or a standard for the noun. An aspect of this subtle semantic alteration can be seen in the following contrasts of position of words with meanings similar to ago:

- | | |
|------------------------|--------------------|
| (28) two earlier years | two years earlier |
| two previous years | two years previous |
| two later years | two years later |

Before the noun, the adjectives mean ANY two years; after the noun, they mean only the IMMEDIATE two years, before or after.

Since ago is a deictic, it can have only the immediate sense, so it follows that it will not occur before the noun.

The deictic status of ago can also throw some light on its lack of stress. Since it differs from back in this regard, and back is not deictic, the best comparison will be with another deictic of comparable meaning, one oriented to the future. One possibility is hence; but hence is not deictic. The deictic contrast is not a single word, but a phrase: from now. In my speech, ago and from now both lack stress:

(29) I went on vacation two weeks ago.

(30) I'm going on vacation two weeks from now.

Both ago and from now also fail to occur in predicate position; this characteristic is thus also not unique to ago.

Ago can also be followed in the noun phrase by additional modifications, a circumstance that also lessens its similarity to a suffix.

(31) You forgot what happened fifteen years ago today, didn't you? (Blondie)

(32) "So, a week ago Sat'day Mr. and Mrs. Pike moved in." (Eudora Welty)

(33) It was fifty years ago this month that Lindbergh poured skill, courage, and determination into a New York to Paris flight that many thought impossible. (William A. Patterson)

(34) A year ago at this time Doyle was hitting around .217... (Larry Whiteside)

(35) It was a year ago on Tuesday. (Mike Littwin)

(36) John left a week ago (from) yesterday. (cited by Hornstein 1977, 532)

These modifiers shift ago from an immediate now, but they are still anchored by the assumed now. Although I have less than full confidence in my judgments about "normal" stress, it seems that ago and from now, in spite of their structural asymmetry, have similar stress patterns:

(37)	a week ago	a week from now
	five years ago today	five years from today
	a week ago (from) Monday	a week from Monday
	a year ago at this time	a year from this time

A difference between ago and from now is that for me and perhaps Bolinger (but not some of my friends), ago cannot have contrastive stress, while from now can. But from now also contrasts directly with from then/today/Monday, while contrasts involving ago are in the added construction: from today, at this time.

Even Bolinger's comment on the metaphorical status of a grief ago is mechanistic. For the mechanist, the distinction between literal and metaphoric is sharp, the latter sometimes treated as if it violated the rules of the language. For the holist, literal-metaphoric is also a matter of gradience. I don't have enough examples to make my point convincingly with ago, but the following selection shows a gradual shift from clearly temporal words to those only inferentially so:

- (38) The officials say that months ago Cuba appealed for milk powder... (Lloyd Shearer)
- (39) Calculating where a planet was three millenia ago is no more valid than calculating where an artificial satellite was three decades ago. (Lynn E. Rose)
- (40) "He died two nights ago..." (Leslie A. Fiedler)
- (41) ...mechanical problems kept him from making any more qualifying runs two Saturdays ago. (AP)
- (42) Everybody remembers Mexico City two springs ago... (Georgie Anne Geyer)
- (43) A generation ago deviant sentences were regarded as material for literary critics and psychoanalysts, not for serious discussion in linguistics. (Dwight Bolinger)
- (44) Eleven starts ago, Thomas was hitting .212... (Bob Stevens)
- (45) One hundred innings ago, left handed pitcher Frank Ferroni of Mill Valley, Calif., didn't think he was ready for professional baseball. (AP)

We can add summers, semesters, weekends, trips, pregnancies, marriages, jobs, heart attacks, martinis, paragraphs, LACUS meetings, etc. While Dylan Thomas' a grief ago is striking, its unusual cast results mainly from our being consciously unaware of all the possibilities in the language.

Bolinger has certainly picked an unusual word for an example. Its lack of a syntactically parallel contrast partner means that ago is unique. But this uniqueness is the result not of a lack of order but of a unique combination of regularities that govern ago. This is a central principles in holistic thought: a complex order makes uniqueness possible. Its syntactic limitations are such that ago can be considered an adjective or an adverb, so that distinctions in parts of speech are also gradient. But ago does not support Bolinger's idiomatic point of view. If a speaker relies mainly on storing constructions with ago, as Bolinger claims, he must store a great many of them, and if he doesn't make the generalizations that I have noted he is limited in his knowledge and potential use of the language.

I caution again that my claims are limited. I am casting doubt on Bolinger's conclusions, not making final judgments of my own. My argument has implied that no safe decisions can be reached on anything with the small number of examples considered in this paper. A holistic view assumes a highly complex order. Where we see only one governing principle, there may be many. An explanation that seems secure today may seem so simply because our limited awareness and methodological laziness make us blind to negative data. We chronically overestimate our ability to marshall all the relevant data. Our data base is still incredibly small, and when it is chiefly introspective, it is heavily tainted -- as any seasoned experimenter, any thoughtful and perceptive person, can readily attest.

I am not claiming that with more data, answers will automatically come; Chomsky is right in his repeated insistence that

we must always go beyond the data. But without sufficient data, the theory has too much to do; the logical and logistic demands of the theory tend to supply more of the data than the language itself. In support of his argument for innate ideas, Chomsky has stressed the structural characteristics of language. But while structure is unmistakable, our particular interpretations of it may be radically misguided. Again, only sufficient data, on a much larger scale than present, can give us any assurance that we are interpreting correctly. The same point can be made for deductive methods. Chomsky stated in Syntactic Structures (1957) the need for such methods:

- (G) Precisely constructed models for linguistic structure can play an important role, both negative and positive, in the process of discovery itself. By pushing a precise but inadequate formulation to an unacceptable conclusion, we can often expose the exact source of this inadequacy and, consequently, gain a deeper understanding of the linguistic data. More positively, a formalized theory may automatically provide solutions for many problems other than those for which it was explicitly designed. (p. 5)

This point should not be underestimated or foolishly ignored. But, like notions of efficiency, such procedures are only as good as the appreciation of relevant factors. When too much of reality is factored out or ignored, both deductive methods and efficiencies become heavily prescriptive and destructive. Further, we tend to overestimate the self-corrective aspects of formalizations; they, like efficiencies, can develop strategies of survival unrelated to the facts. The dangers are especially great when much of the reality is unknown; and with our current misguided trust in our conscious awareness, the dangers are real.

Bolinger's judgment is that Transformational Grammar is reductive; and he is right. Language is more orderly, more complex than even TG claims. The best compliment that can be given Dwight Bolinger is that he had fashioned the foundations of a holistic revolution, and that, like Joseph Conrad, he has been teaching us to see. But he has been unable to see the whole picture. He has seen all the little details, all the individual uniqueness and complexity, and he has known that TG has ignored much. But he has not considered that an even greater order lies behind all this diversity, and has instead championed an idiosyncratic view, which is also reductive. There may be considerable disorder in language, perhaps more than we suspect. But if we have undervalued anything, it is the order of language, and all the factors of human life and knowledge that create that order. If we follow Bolinger's advice in (C), we will gradually see the full dimensions of that order. That advice is a credo. Without those attitudes and methods, Bolinger's vision will remain clouded, and there will be no holistic revolution.

NOTES

1. Although this paper is critical, I hope it is obvious that I owe Dwight Bolinger a considerable debt. My training has been in Transformational Grammar, and it can be considered my parent. But Dwight Bolinger has been like a beloved uncle who reveals aspects of life that my parent either ignores or does not know. For those familiar with his work, I hope it is also obvious that in criticizing Bolinger I am using his methods. I would like to think that if someone else had written "Meaning and Memory," he would have made the same criticisms.
2. There is a reductive quality to this characterization of TG. Generativity is not limited to phrase structure rules, as Bolinger's discussion seems to imply. Also, the determinate analysis of wholes into parts is a Structuralist technique; it has been partly adopted by TG, but TG's reason for being is that such analysis is by itself inadequate. For that matter, Structuralists have not limited themselves to these methods.
3. There is a systematic alteration of this status in fiction where by convention "present" is in past tense.

BIBLIOGRAPHY

- Antilla, Raimo (1972), *An Introduction to Historical and Comparative Linguistics*. New York: Macmillan
- Bolinger, Dwight (1952), "Linear modification," *Publications of the Modern Language Association* 67:1117-44. Also in Bolinger, Dwight (1965), *Forms of English: Accent, Morpheme, Order*, eds. Isamu Abe and Tetsuya Kanekiyo. Cambridge, Mass.: Harvard University Press
- Bolinger, Dwight (1965), "The atomization of meaning," *Language* 41.4, 555-73. Also in Jakobovits, Leon A. and Murray S. Miron, eds. (1967), *Readings in the Psychology of Language*. Englewood Cliffs, N.J.: Prentice-Hall, Inc. Page references to latter.
- Bolinger, Dwight (1967), "Adjectives in English: attribution and predication," *Lingua* 18, 1-34
- Bolinger, Dwight (1971), "Semantic overloading: a restudy of the verb remind," *Language* 47.3, 522-47
- Bolinger, Dwight (1976), "Meaning and Memory," *Forum Linguisticum* 1.1, 1-14
- Chomsky, Noam (1957), *Syntactic Structures*. The Hague: Mouton
- Katz, Jerrold J. and Jerry A. Fodor (1963), "The structure of a semantic theory," *Language* 39.2, 170-210
- Postal, Paul (1970), "On the surface verb remind," *Linguistic Inquiry* 1.1, 37-120
- Ruhl, Charles (1977), "Idioms and data," *The Third LACUS Forum*, eds. Robert J. DiPietro and Edward L. Blansitt, Jr. Columbia, S.C.: Hornbeam Press, Inc.

PARADIGMS AND REVOLUTIONS IN LINGUISTICS

Bruce L. Pearson
University of South Carolina

Since it first appeared in 1962, Thomas Kuhn's book The Nature of Scientific Revolutions has generated extensive discussion among historians of both the natural and social sciences. Kuhn's initial impact came in challenging the popular notion that scientific advances come in increments as investigators make discoveries that add bits and pieces to an ever accumulating body of knowledge. His position was that scientific advances, instead of coming in a piecemeal fashion, come about as the revolutionary insight of a gifted individual whose research findings resolve old problems and provide a framework for attacking new problems. These highly original contributions amount to a breakthrough in the handling of scientific issues or, in Kuhn's terminology, a scientific revolution.

In any field of scientific inquiry there are always many topics that can be studied in a rather routine way. But there are other perplexing topics that are more or less intractable, topics that simply do not lend themselves to resolution within the prevailing framework of assumptions and research procedures available to investigators. As these unresolved issues accumulate, science enters a crisis period and remains in a state of crisis until a gifted individual appears whose original thinking and novel approach touches off a revolution within the scientific community. The impact of this revolution is twofold. On the one hand it provides a satisfactory way of dealing with perplexing issues that had formerly been anomalies; and at the same time, in integrating the former anomalies into a pre-existing body of knowledge, it offers a new way of viewing familiar material and produces in effect a reinterpretation of the entire field. The result is a totally new framework for viewing the field of inquiry, complete with new assumptions about the nature of the field and new research methodologies growing out of these assumptions.

Kuhn originally called the framework for viewing a field of inquiry a scientific paradigm. In an appendix to the second edition of his book (1970) he proposes the term disciplinary matrix. Since both terms are in use in the literature discussing Kuhn, I will regard them as interchangeable labels for the same concept.

One characteristic of a scientific revolution is that, while it resolves numerous anomalies and reinterprets the conventional wisdom of the field, the paradigm that results from it channels the efforts of investigators to new and previously inaccessible problems calling for their attention. Although some established scholars may resist the new ideology, the novelty and potency of the new paradigm is attractive to younger scientists who are still in the process of establishing themselves and who may be inclined to reject the assumptions and methods of older scientists, whose

work strikes them as having been superceded by the new approach. Except for holdouts here and there, the new paradigm is embraced by the entire scientific community. Investigators enter what Kuhn calls a period of normal science, in which research is guided by the new paradigm and research findings are interpreted according to the assumptions and expectations of the new paradigm.

The appeal of this view of history to many contemporary linguists is apparent as soon as we fill in the cast of characters provided by Kuhn. The gifted individual is Noam Chomsky. The scientific breakthrough is represented by his innovative work in Syntactic Structures and subsequent publications. The new paradigm is transformational grammar.

Admittedly, many of the observable facts conform to those that might be predicted on the basis of Kuhn's model. Large numbers of younger linguists have been attracted to the Chomskyan paradigm. For them transformational grammar has provided a framework for the scientific investigation of language. For many, both within and without our discipline, linguistics and transformational grammar have become synonymous, interchangeable terms. But in certain respects Kuhn's model seems to fall short of adequately describing the contemporary scene in modern linguistics. We might expect a few scholars nearing their dotage to resist the new paradigm after twenty years, but in fact substantial numbers of linguists continue to practice structural grammar or historical analysis and either ignore transformational grammar or regard it---even after twenty years---as a transitory phenomenon.

In addition, we have a variety of competing schools, including tagmemics, stratificational grammar, and meaning-structure grammar that continue to be offered as alternative paradigms. These paradigms continue to attract adherents and contribute to scholarly discussion although they are consistently upstaged by the transformational school. Moreover, the acrimony within transformational grammar itself, with an ongoing rivalry between the approaches known as interpretive semantics and generative semantics, is such that one can hardly say the linguistic community has subscribed to a single viewpoint that serves as a universally accepted scientific paradigm. In fact, it is only a slight exaggeration to say that the prevailing approach requires each practitioner to devise his or her own scientific paradigm, which can then be offered as a rival to all others.

This state of affairs has led some critics, notably Keith Percival (1976, 1977), to claim that Kuhn's model is inapplicable to linguistics. Although Percival's published remarks are confined to the inability of Kuhn's model to account for modern linguistics, one supposes that Percival has serious reservations about the validity of Kuhn's hypothesis to account for scientific progress in general.

Granted that the current scene is not as homogeneous as one might expect on the basis of Kuhn's predictions, we must still ask whether the overall history of linguistics fits into the broad outlines of Kuhn's theory.

Traditional grammar, originating in Greece and transmitted via Rome to Medieval Europe, served as the only available linguistic paradigm until roughly the beginning of the nineteenth century. The interests of traditional grammar were actually quite diverse. Historians of linguistics are only now beginning a serious investigation of the varied strands of thought that comprise what we call traditional grammar. It is therefore difficult at this point to say exactly how the different movements that can be identified at different periods within traditional grammar thought of themselves. However, Bursill-Hall's observation (1974:81) that some of the earlier Modistae refer to an earlier scholar, "somewhat disparagingly, as one of their predecessors" suggests that academic behavior has not changed greatly over the years.

It is perhaps human nature to take for granted the factual knowledge transmitted to us from earlier generations and to attach disproportionate significance to our own contributions. It is perhaps natural to believe that our own contributions are highly original, go beyond the work of our predecessors, and establish order order where formerly there was chaos. But holding such a belief—even expressing it in public—does not in itself make one's work revolutionary. Thus, even if studies now in progress eventually show that all the varied movements associated with traditional grammar thought of themselves as offering revolutionary contributions, the fact remains that from the perspective of the present day all of these movements until the nineteenth century seem to be part of an ongoing effort to pursue a single goal—the synchronic analysis of individual languages using the analytical tools, with only minor refinements, that date back to the Greeks.

It was not until the beginning of the nineteenth century, with its shift of emphasis to diachronic studies, that a movement of genuinely innovative significance is discernible. Even this boundary is somewhat arbitrary and retrospective. Interest in the origin and prehistory of language as well as the comparative study of individual languages is found before the beginning of the nineteenth century, and some of these early studies anticipate certain details of the studies undertaken later by comparativists of the nineteenth century (Hymes 1974). However, it was the widespread acceptance of historical studies and the complete domination in academic circles of comparative linguistics that mark the nineteenth century brand of linguistics as a revolutionary departure from language studies up to that time. The nineteenth century gave rise to the concept of genetic relations among languages, the study of sound change, and the science of phonetics. Still, it is important to recognize that throughout the nineteenth century linguists were schooled in the classics and took traditional grammar for granted. Traditional grammar remained the foundation for the study of language throughout the nineteenth century and indeed continues to dominate the formal study of language at most levels in our schools even to the

present day. It is evident that the rise of historical linguistics did not lead scholars to abandon traditional grammar, but it does seem reasonable to suggest that the practice of traditional grammar has been modified in various ways as a reaction to historical grammar and to the movements of the present century. A closer look at modern varieties of traditional grammar will be a necessary part of a complete history of linguistics.

The best known of the self-proclaimed revolutionary movements within linguistics before the present century was of course the Junggrammatiker movement of the 1870s. But this movement, in retrospect, is so clearly a continuation of the interests and methodology of the earlier comparativists that present day undergraduates have difficulty even understanding the refinements of methodology introduced by the movement. Admittedly, part of this difficulty comes from our tendency to see the work of Grimm and his contemporaries in terms of our own understanding rather than in terms of their more limited perspective. Inevitably, from the vantage point of the twentieth century it is the initiators of the movement that we identify as the revolutionary innovators and not the later giants who refined the methodology.

Structural grammar in the twentieth century, with its shift from diachronic to synchronic studies, genuinely represents a second revolution and introduces a third paradigm. The interests and goals are similar to those of traditional grammar, but the methodology is largely inherited from historical grammar. The transition came about gradually and more as a shift of emphasis than an outright break with the immediate past. At no time did linguists abandon diachronic studies. In fact, the present century has undoubtedly witnessed more diachronic studies of a greater variety of languages than was the case in the supposed heyday of historical studies in the last century. The polemic denunciations issued by structuralists were aimed not at historical linguists, with whom there was no danger of confusion anyway, but at traditional grammarians, whose interests were close enough to their own that structuralists felt constrained to set forth the differences at every opportunity.

It is difficult to identify a single individual as the founder of structural grammar although Kuhn's model seems to require some kind of founding figure. Possibly Ferdinand de Saussure could be regarded as such a person, but in reality his influence was not felt until some time after the structural movement was already established (Percival 1977:393). Leonard Bloomfield is perhaps a better candidate for the role of founding figure since his book Language (1933) did more than any other single work to establish a matrix for the scientific study of language for an entire generation of linguists. Bloomfield's Language is a highly original work, but at the same time it must be understood as an outgrowth of intellectual currents such as behaviorism and the synchronic study of non-Indo-European languages that did not themselves originate with Bloomfield. Saussure's popularity came after structuralism had become a

self conscious movement and structural scholars had begun a quest for a theoretician earlier than Bloomfield with whom they could identify. The fact is that there is no particular date on which a single individual announced the launching of a new movement and determined to call it structuralism. Yet, as we near the end of the century, we begin to see retrospectively that the entire century has been dominated by an approach that is conveniently summed up by the term structuralism--that is, the synchronic study of individual languages in terms of their own structural categories.

Seen in this light, the transformational school of the last twenty years must be viewed as a twentieth century Junggrammatiker movement. This movement has developed useful tools for analyzing syntax, and these tools will no doubt take their place alongside methods developed by earlier structuralists for analyzing phonology and morphology. Transformational grammar has also opened the door to semantic analysis although there is certainly no consensus at present as to just how semantic analysis should be carried out. This is the very issue over which we find the greatest disagreement among transformationalists and the greatest variety of models being proposed as alternative vehicles. It seems likely, however, that in the years remaining in this century some kind of consensus will emerge on this point, and future historians will no doubt see the twentieth century as a relatively unified period dedicated to the single task of developing a model for the synchronic analysis of language. By that time linguists will surely be turning their attention to other matters that will seem more pressing to citizens of the twenty-first century, and we can expect this new focus to give rise to something that will eventually come to be regarded as a new paradigm.

If this happens, I would guess that the paradigm will grow out of currents that have already begun to emerge. Among the currents that might capture the allegiance of investigators and acquire the status of dominant paradigm are neurolinguistics or computational linguistics or even some hybrid that we can only at present imagine--say, neurocomputational linguistics, the use of computers to simulate the program of the nervous system and communicate with humans using the language of the nervous system itself. Such a development would make linguistics of the future seem quite different from present day linguistics but would be no more extreme than the development of phonemics from earlier phonetics or the development of transformations from earlier distributional studies of morphosyntactic structures. Of course, even if we find some new paradigm achieving dominance in the next century, we can expect on the basis of past experience that many linguists will continue to follow their own interests in more traditional historical or descriptive studies and will remain uninvolved in the new movement.

Although I am inclined to accept Kuhn's thesis in broad outline, my acceptance is so hedged with qualifications that Percival, I am sure, would regard it as outright rejection.

Still, the concept of a scientific revolution and an attendant scientific paradigm is useful and remains broadly applicable to linguistics, with the following provisions: (1) The participants in a supposed revolution are not in the best position to appreciate the significance (or lack of significance) of their own work. (2) Revolutions in linguistics may differ from other scientific revolutions in that older paradigms (e.g., historical linguistics) can retain most if not all of their validity and continue to be practiced alongside the newer model.

The first of these points is recognized by Kuhn's critics in the natural sciences and the philosophy of science (cf. Lakatos 1970:162, 173-174) and is certainly compatible with Kuhn's own observations (e.g., 1962:54). The second point is perhaps more crucial since Kuhn sees scientific progress as a successive series of revolutions while his critics minimize the scope of these revolutions and interpret scientific progress as consisting of successive shifts in emphasis and/or viewpoint describable as gradual evolution, not revolution at all. Toulmin (1970) suggests that, when proper allowances are made for the rhetorical stance adopted both by Kuhn and those who argue for gradual evolution, the two positions become indistinguishable. The evolutionists are forced to recognize the existence of a great many mini-revolutions with attendant discontinuity, but Kuhn and his supporters are compelled at the same time to recognize that much continuity of thought bridges the supposed revolutions.

Curiously enough, earlier students of the history of science seem to have recognized more clearly the mixture of evolutionary and revolutionary elements in scientific progress than do contemporary scholars. Thus Philipp G. Frank (1884-1966), generally considered an evolutionist, describes geometry as "a fairly stable science" but immediately contrasts it with mechanics, which "reveals great differences between modern mechanics (since the seventeenth century) and earlier theories" (1957:90-91). Frank avoids using terms like "evolution" or "revolution," preferring instead to trace the development of ideas in all their complexities. For example, in discussing the replacement of the Ptolemaic conception of the universe with the Copernican conception, he notes that acceptance of the Copernican system, despite its obvious superiority even by scientific standards of the day, "did not happen before a long period of doubt," which he relates to the general intellectual climate of the period, noting that "physical science is only a part of science in general," which in turn is "a particular act of human behavior" in general (1964:14-15).¹

The fact is that we find some features of both an evolutionary and revolutionary model present in the development of linguistics. An evolutionary model assumes variation at every state of development. Some variants thrive while others falter. The more successful variants may in some cases displace those that are less successful, but competing variants will always exist side by side. A revolutionary model differs primarily by paying

less attention to the overlap of competing variants and stressing instead the apparent discontinuity that occurs when one paradigm supercedes another. In linguistics, the rise of a new paradigm, while widely hailed as a revolutionary event, has commonly been marked by much not-so-widely heralded continuity. One reason may be that what is taken to be a revolutionary paradigm is actually less revolutionary--and a good bit less comprehensive--than is generally supposed. Thus we find that insights from an earlier period remain valid and continue to serve the research needs of many investigators. A great deal of synthesizing is still needed to develop a disciplinary matrix for linguistics that is comprehensive enough to encompass synchronic, diachronic, typological, and developmental studies. But until the synthesis has been achieved, it seems wise to defer extravagant claims about the virtues of one paradigm and the failings of others.

FOOTNOTE

¹ I am indebted to H. A. Gleason for bringing Frank's work to my attention.

REFERENCES

- Bloomfield, Leonard. 1933. Language. New York: Holt.
- Bursill-Hall, G. L. 1974. "Toward a History of Linguistics in the Middle Ages, 1100-1450," 77-92 in Hymes 1974.
- Frank, Philipp G. 1957. Philosophy of Science. Englewood Cliffs: Prentice-Hall.
- _____. 1961. "The Variety of Reasons for the Acceptance of Scientific Theories," 13-25 in Philipp G. Frank (ed.) The Validation of Scientific Theories. New York: Collier Books, 1961 (reprinted from the September, October, November 1954, and January and February 1955 issues of The Scientific Monthly).
- Hymes, Dell (ed.). 1974. Studies in the History of Linguistics. Bloomington: Indiana University Press.
- Kuhn, Thomas S. 1962, 1970. The Nature of Scientific Revolutions. Chicago: University of Chicago Press.
- Lakatos, Imre. 1970. "Falsification and the Methodology of Scientific Research Programmes," 91-196 in Lakatos and Musgrave 1970.
- Lakatos, Imre, and Alan Musgrave (eds.). 1970. Criticism and the Growth of Knowledge. London: Cambridge University Press.
- Pearson, Bruce L. 1977. Introduction to Linguistic Concepts. New York: Alfred A. Knopf.
- Percival, W. Keith. 1976. "The Applicability of Kuhn's Paradigms to the History of Linguistics," Language 52.285-294.
- _____. 1977. Review of E. F. K. Koerner's Ferdinand de Saussure. Language 53.383-405.
- Toulmin, S. E. 1970. "Does the Distinction Between Normal and Revolutionary Science Hold Water?" 39-48 in Lakatos and Musgrave 1970.

FORMAL ASPECTS OF STRATIFICATIONAL THEORY (1)

Alexander T. Borgida
University of Toronto

1. Introduction

Linguistic theories should be judged on the basis of their ability to usefully present actual linguistic data. However, since the advent of the Transformational Generative approach, it has become necessary to consider the formal aspects of competing theories as well. The purpose of this note is to outline one possible formal model for the stratificational theory of linguistics, and to report on a number of formal meta-theoretical results. We examine these results in light of pseudo-formal claims made about stratificational grammars (SGs) in the past, and we provide evidence about aspects of current linguistic practice which are likely to cause problems when stratificational analyses will be put to practical use. It is assumed that the reader is familiar with the stratificational approach, and the often-used notational conventions (Lambograms) (e.g. see Lockwood(1972)). Note however that in this research the purely stratificational aspects of the theory have been divorced from the "relational" ones, and the terms "stratificational grammar and theory" are used in the second sense described by Algeo (1968).

2. A formal model

Consider the basic principles of the stratificational approach: (1) The data about natural language regularities can be presented more advantageously if divided into subsystems (strata), each including its own inventory of fundamental units, and a set of rules (the tactics) which specifies the well-formed structures on that stratum. (2) Furthermore, the strata are linearly ordered as levels and adjacent strata are connected by the realization relation, which attaches to every well-formed structure on some stratum one or more accompanying structures on the adjacent levels. If we consider the tactics as describing the range of possible structures by providing options at various points, then realization can be given expression as restrictions on these options, the restrictions being statements about structures on the neighboring strata. A significant result of the above considerations is that a particular utterance has simultaneous expression on each stratum and, unlike in other models, one stratum does not process the output of another stratum.

This description suggests that the tactics play the role of a "syntax" of legitimate constructions on some stratum, and hence that rewrite (phrase-structure) grammars can be used to model the tactics. Among the reasons for this is the precedent set by Sampson (1970), and the fact that the formal properties of rewrite grammars have been thoroughly studied in the past, thus providing a solid basis from which to investigate the consequences of the truly novel idea in SG: the realization. We therefore model the tactics by a grammar which consist, as usual, of two sets: the terminals and the nonterminals, an initial symbol S , and a set of rewrite rules or productions. The grammar specifies a language of "correct" strings by the usual generative method of derivation: replacing a sub-string of nonterminals x in some sentence by a string y of terminals and nonterminals, if $x \rightarrow y$ was one of the productions of the grammar; this proceeds until there are no more nonterminals left. One can now view an SG (with two rewrite grammars for tactics) as producing sentences in the following manner: derivations begin on each stratum from the corresponding start symbol, and proceed in parallel until each one terminates; since the resulting sentences would normally be unrelated, the realization must ensure that the two derivations are coupled together somehow. Realization will therefore be formalized as a relation connecting the productions of the two grammars corresponding to the tactics, and the derivations on the two tactics will be constrained so that only related productions may apply "simultaneously". This model is of the type called "tactics-tactics" by Gleason (1976), and is the basis of the following:

Informal definition: A stratificational grammar with two strata consists of two rewrite grammars G_1 and G_2 , representing the tactics of the two strata, and a relation R , corresponding to the realization. R is a finite set of ordered pairs of the form (p, q) such that p is a production in G_1 and q in G_2 . The derivations in G_1 and G_2 will then be related so that if production p applies in the derivation of a sentence according to G_1 , then only those productions q may apply at that point in the G_2 -derivation which are R -related to p (i.e. $(p, q) \in R$). The language generated by the SG is then the set of terminal strings generated by G_2 in this way. # This model allows us to represent one-one realization, neutralization and diversification (see Fig.1). In order to handle the other discrepancies, the definition is extended so that R may contain ordered pairs of the form (f, g) , where f and g are finite strings of productions from G_1 and G_2 respectively (e.g. see Fig.2).

Finally, SGs with three or more strata can be defined as the natural extension of the two-stratal case, by using rewrite grammars for tactics, and realization relations between each pair of adjacent strata.

One of the advantages of formalization is that in the process one often uncovers ambiguities in the original linguistic theory. One instance in our current model is the manner in which derivations proceed on the tactics: at any point in the derivation there may be many substrings equal to the left-hand sides of productions and the question arises as to which ones can be rewritten. If there are no restrictions at all, then the derivation will be said to be free. One common constraint is to require the leftmost nonterminal occurring in a sentence to be part of the substring replaced; this is known as the leftmost derivation constraint. A slightly more relaxed version of this is to allow any nonterminal within some fixed number k of symbols from the leftmost one to be rewritten: k -leftmost derivation (2). Both of these constraints have strong intuitive appeal as models of language production: creating sentences from left to right in the temporal order of speech, when compared to the random, haphazard choice allowed when no constraints are imposed. We will assume henceforth that all derivations are leftmost.

The above is clearly a "generative" definition of the stratificational model. We give here a tentative description of the way in which such a model could be used to encode sentences. The meaning of an utterance could be specified by the tree or graph structure representing some "correct" derivation on the upper tactics. Gleason (1973) then sees a "realize center" moving about this structure, and depending on conditions satisfied around this center, realization ensures that one or another alternative production is applied on the lower tactics to produce the encoding. In our case, we may re-interpret the "application" of a production on the upper tactics as the checking of certain conditions in the derivation tree. (e.g. production $A \rightarrow BC$ applied iff node A has descendents B and C). Such a description could be specified by some type of tree automaton for example. The leftmost derivation constraint then would provide guidance to the movement of the realize center through the semologic structure, and if production p applied in the "checking" sense, then a production q such that $(p,q) \in R$ could apply in the lower tactics.

3. Some basic formal notions

It is assumed that the reader is familiar with the elementary concepts of formal language theory such as rewrite grammars and productions, finite-state (f.s.), context-free (c.f.) and context-sensitive (c.s.) grammars and languages (e.g. see Chomsky & Miller (1963)). We will also need the following less well-known terms. Suppose G is a c.f. grammar and let xAy be a sentence derived from A using rules of G . If both x and y can be non-null then G is said to be self-embedding (s.e.); if in all such strings xAy , y

must be null, then G is right-recursive (r.r.), and if x must be null then G is left-recursive. The notion of s.e. is important since Chomsky has shown that it this property which allows grammars to generate non-f.s. languages, while Reich has argued that natural languages do not really contain centrally embedded constructions. On the other hand, right recursion is of interest because almost all mini-grammars in the stratificational literature have tactics of this type.

One final important distinction arises between the recursive (REC) and recursively enumerable (RE) languages: for the former there exist effective procedures (finite, unambiguous, mechanically executable sets of instructions) which determine in a finite number of steps whether any sentence does or does not belong to the language; for the latter, one is guaranteed only a procedure to enumerate sentences (i.e. there may be no "parser" which rejects incorrect sentences, as well as accepting correct ones). We say that the "membership question / problem for RE sets" is undecidable / unsolvable. The distinction between RE and REC is important for several reasons:

- there exist RE sets which are not recursive;
- most linguists believe that natural languages are recursive;
- for practical applications such as natural language communication with computers, one will have to construct parsers from linguistic grammars.

We are now ready to consider some of the formal results which have been proven about the SG model.

4. Generative power of SG

Formal results about generative power have been used in the past to label linguistic theories as too "weak" (Chomsky (1957), Postal (1964)), or too "powerful" (Peters & Ritchie (1973)). In the case of SG with two or more strata, we have demonstrated the following results:

- if all tactics are r.r. grammars, then the SG can generate only f.s. languages;
- if one of the tactics is allowed to be non-r.r., then the SG can generate c.f. languages;
- if at least two tactics are non-r.r. (i.e. are left recursive or self-embedding) then the SG can generate non-c.f. languages, such as { $w\#w$ }, where w is any string over an alphabet.

These results are significant for several reasons: (1) They show that stratification does not increase the generative power of the individual tactics if these are r.r., but it does so otherwise. (2) If one wishes to avoid generating centrally-embedded sentences with SGs, then it is not sufficient to eliminate only self-embedding from the tactics; one must also eliminate left-recursion. (3) The existence of 2-stratal grammars with c.f. tactics which generate non-c.f. languages discredits Postal's

classification of ST as just another variant of c.f. phrase structure grammars (Postal(1964)).

Further study has shown that SGs with c.f. tactics can in fact generate all RE sets! Therefore the step which we have taken in order to generate some context-sensitive languages appears to be too large, since it opens stratificational theory to the same criticisms as leveled against transformational theory. It was found that in order to generate non-recursive sets with SGs, one requires the concurrence of two conditions: at least two non-right-recursive tactics, and arbitrary use of either length-decreasing rules on the lowest tactics, or of zero realizations of tactics-terminal units. The excessive power of SGs can be largely attributed to the latter conditions. This can be seen from the fact that if the use of length-decreasing rules and of null realizations is restricted, then SGs generate only a subclass of the context sensitive languages. This class still contains ncn-c.f. languages such as { $w#w$ } but has a solvable recognition problem.

The above theorems place length-decreasing productions in the bottom tactics and null realizations of terminals, in the same position as that held by deletion transformations in TG theory: their use has to be curbed in order to avoid generating non-recursive languages. This suggests several guidelines for linguistic usage: (1) Null rules (null option) should be avoided as much as possible in the lowest tactics; if used, ensure that no more than some fixed number of null productions can apply successively, before a non-null symbol is generated. (2) Similarly for zero realization. (3) Besides null rules, some linguists have used "UPWARD AND" relations, similar to productions $AB \rightarrow C$ (which are length decreasing), in order to express context-dependent linguistic phenomena in the tactics. On the basis of our results, a preferable alternative is to use "environmental conditioning" (" $B \rightarrow C$ if A occurred") in the tactics and reserve UPWARD ANDs for realization rules; this approach seems to be quite feasible and has been in fact followed by Lockwood (Lockwood(1970)).

In conclusion, the above results establish stratificationism as a formally viable alternative to transformationalism, even if it is agreed that c.f. grammars are inadequate for describing natural languages.

A second purpose of formal results about generative power is to compare alternate proposals in linguistic theory, and thus provide linguists with some insight into the effects of their choices.

For example, we have demonstrated that the addition of context-dependent productions to c.f. tactics does not increase generative power in our model. This contradicts the intuition voiced by some linguists (e.g. Sampson(1970:11)) that the use of context-sensitive

relations (such as UPWARD AND) leads by itself to the ability of describing non-c.f. constructions. On the positive side, the above result indicates that such devices as the "environmental conditioners" ("enablers") of Lockwood, or the "permutation rules" proposed by Gleason, can be freely used without increasing the generative power of SGs (and thus making them too powerful), while at the same time providing a simpler treatment of linguistic data.

We have also shown that in many cases SGs with n strata ($n \geq 4$) have the same generative power as SGs with 3 strata; therefore the decision to use 4 or 5, versus 3, strata in linguistic descriptions cannot be made on the grounds of formal generative power.

5. Ordering rules in SGs

The idea of "priority" in applying productions or realization rules arises quite often in linguistic rules where there are so called "unmarked alternatives". There are numerous examples of this, especially in the phonotactics, or in realization when one wishes to express the fact that an entity on the upper stratum is realized one way if some condition holds, and a different way otherwise. For example, the past tense in English is normally realized by the suffix /+ed/ following the verb, but for the "irregular" verbs (e.g. "run") we have to use different forms.

In Lambograms, this is expressed by the ORDERED OR relation. Unfortunately, its use is extended so that in the most general case its meaning seems to be equivalent to the rule "A->B if this leads to a successfully completed derivation; otherwise A->C". In the case of a single c.f. network, this relation is superfluous since it is decidable beforehand whether a terminal string can be derived from B or not. On the other hand, in networks with c.d. relations and free derivation, or in n-stratal grammars which can generate non-c.f. languages, the question of completing derivations becomes undecidable. This means that grammars employing ORDERED-ORS as defined above cannot be the basis of effective procedures for even enumerating all the sentences in a language, let alone of algorithms for parsing their sentences. Therefore, such grammars are highly undesirable since they cannot be said to characterize languages in any useful way. The same criticism applies to the definition of ORDERED-OR given by Sampson, where the choices are given weights (preference values), which are accumulated to the end of the derivation, at which time the sentence with the maximum weight is chosen.

In the model presented in this paper, such descriptions can be implemented by partially ordering the realization rules, so that some rules have precedence over others. This means that one can specify that some realization rules should be attempted before others, if several are

simultaneously applicable. The important difference between this definition and the previous ones is that the rule to apply is selected solely on the basis of the current state of the derivation, and the precedence of the rules, and is not influenced by the possible success or failure of the derivation later on.

The significant formal result about this extension is that it does not increase the generative power of SGs, and thus has the same status as c.d. rules in tactics or realization.

Note that different orderings may be assigned to the realization rules depending on whether the grammar is being used for encoding or decoding utterances. This is one possible answer to the problem of realizational assymetry in encoding and decoding, as posed by Gleason (1973).

6. Grammatical complexity

Following Lamb, many stratificationalists have adopted as a second basic tenet the view that language structure consists entirely of relationships, connecting only on its periphery with "objects" such as meaning and sound. Furthermore, these relationships can be described using only a small set of relationship types, normally represented by nodes (e.g. AND, OR, etc.) connected into a network. One of the important consequences of this representation of grammars by networks is the fact that a simple, uniform evaluation measure can be defined on them: namely, the count of nodes used (3). This measure enables one to decide which of two networks representing the same data is simpler, and hence constitutes a better description. In order to avoid unnecessary complications, we have considered only networks containing the three "c.f. relations": DOWNWARD ORDERED AND, DOWNWARD UNORDERED OR, UPWARD OR. We summarize here the results of our investigation of this evaluation measure.

First, we examined the complexity of c.f. networks without stratification. It was shown that there can be no general algorithm to minimize networks (i.e. given some network N, the algorithm should return N' such that their languages are the same, and the complexity of N' is the smallest possible). Even the weaker problem of deciding whether some particular network is minimal or not is undecidable. This effectively destroys any hope of discovering mechanically even c.f. networks for some set of data, or of minimizing networks automatically. It also casts doubts on Reich's proposal that language acquisition is accomplished through grammar simplification (Reich(1970)).

This unsolvability result is no longer valid if the networks are restricted to generate f.s. languages, as is the case once the nodes are interpreted to be f.s. devices sending and receiving signals (Reich(1970)). Therefore, it may be of interest to examine the simplification of

networks generating f.s. languages, or even more elementarily finite languages. Given a network with no "recursive loops", one could simply enumerate all "simpler" nets, and then test whether they generate the same language. This is of course an impractical approach, and the question then is whether there exists some efficient algorithm to accomplish this reduction process. The answer is no, because we have shown that the minimization problem for c.f. networks without loops is at least as difficult as some other problems which have been proven to be computationally intractable. This essentially means that any computer program would require astronomically long time to minimize even moderate sized networks, thus further weakening the plausibility of the language-learning method proposed by Feich. It is however true that a learning mechanism whereby people do not acquire "minimal" networks is still feasible.

Consider next the properties of the evaluation measure extended to SGs. Since ordinary, non-stratificational networks become subparts of SGs in the form of tactics, then all the properties proven before about the evaluation measure extend directly to the complexity of SGs. An interesting new problem which can however be investigated concerns the relative complexity of representing the same data in terms of single and multi-stratal grammars.

Let r be some fixed positive integer, and consider the language

$$Q(r) = \{ y \mid y \text{ is an } \underline{\text{any}} \text{ string of } \underline{r} \text{ symbols from some finite alphabet with at least two letters} \}$$

In other words, $Q(r)$ contains all sentences consisting of two identical subsentences, each of length r . Note that $Q(r)$ is a finite language (it only has 2 to the power r sentences), and the constructs in $Q(r)$ are similar to sentences of the form

"Joe, Abe and Moe ate a pear, a mango and a pie respectively"
The importance of the language $Q(r)$ lies in the fact that one can show that any network which generates $Q(r)$ will have at least 2 to the power r c.f. nodes in it (up to a constant factor). On the other hand, one can exhibit a two-stratal grammar which generates $Q(r)$, and whose complexity is directly proportional to r . For any values of the constants of proportionality, one can find some value of r such that the complexity of $Q(r)$ is smaller when represented by a SG than by any single-stratal grammar. Furthermore, as the value of r increases, so does the difference between the sizes of the simplest two-stratal and single-stratal grammars describing $Q(r)$.

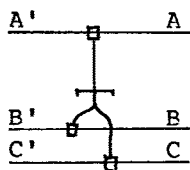
This result provides formal support to the linguists' contention that stratificationalism is a good descriptive strategy since it allows a more succinct representation of linguistic data.

8. Conclusions

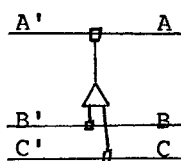
We have presented a mathematical model for stratificational linguistics, and based on this, we advanced two types of formal arguments in favour of the stratificational approach, as a viable alternative to the transformational one. By varying the types of rules allowed in the tactics, and the extent to which zero realization is used, we showed that SGs can generate families of languages ranging from the f.s. languages to the RE sets, thus matching the generative power of TG. From the point of view of complexity, we demonstrated the existence of even finite languages which can be described more economically using 2-stratal grammars than single stratal ones. We also used formal results to point out some problematic areas in current stratificational work, and suggested some possible solutions.

Footnotes.

- ¹ This paper is abstracted from the author's Ph.D. dissertation submitted to the Department of Computer Science at the University of Toronto. I am grateful to Professors C.R.Perrault, H.A.Gleason Jr. and P.A.Reich for their assistance, and to the National Research Council of Canada for financial support.
- ² Figures 3 and 4 illustrate the effect of the various constraints on the sentences considered correct according to some SG networks.
- ³ Without loss of generality, we have assumed that all nodes represent "ternary" relations (i.e. no node has more than 3 "wires" connected to it).

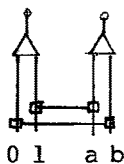
Figures.Fig. 1

Diversification:

$$R = \{(A-A', B-B'), (A-A', C-C')\}$$
Fig. 2

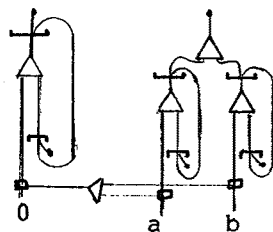
Composite realization:

$$R = \{(A-A', B-B' \ C-C')\}.$$



with 1.d.: none;
with 2-ld: "ab".

Fig. 3&4
Sentences correct according
to the above nets:



with 1.d.: "ab";
with 2-ld: "ab", "aabb";
with free: $\{a^n b^n \mid n \geq 1\}$.

References.

- Algeo, J. (1969). "Stratificational Grammar", Journal of English Linguistics - 3, 1-7.
- Chomsky, N. (1957). Syntactic Structures, Mouton, The
- Chomsky, N. and G.A.Miller (1963). "Introduction to the Formal Analysis of Natural Languages", in Handbook of Mathematical Psychology, Vol.2, John Wiley and Sons.
- Gleason, H.A.Jr. (1973). The Architecture of Language, unpublished manuscript, University of Toronto.
- Gleason, H.A. Jr. (1976). "Realization, Transformation and Filters", The Third LACUS Forum, 1976, Hornbeam Press, Columbia, S.C.
- Lockwood, D.G. (1972). Introduction to Stratificational Linguistics, Harcourt Brace Jovanovich Inc.
- Peters, P.S. and R.W.Ritchie (1973). "On the generative power of Transformational Grammars", Information Sciences - 6, 49-83.
- Postal, P. (1964). Constituent Structure: A Study of Contemporary Models of Syntactic Description, Indiana University, Bloomington, Ind. First appeared in Int.J. Amer. Linguistics - 30.1, part 3.
- Reich, P.A. (1970). "A Relational Network Model of Language Behaviour". Ph.D. Thesis, University of Michigan.
- Sampson, G. (1970). Stratificational grammar: a Definition and an Example, Janua Linguarum, Series Minor: 88, Mouton, The Hague. Hague.

VI

**DIACHRONIC LINGUISTICS:
HISTORICAL AND COMPARATIVE LINGUISTICS**

RELATING SYNCHRONY AND DIACHRONY
IN THE ANALYSIS AND DESCRIPTION OF LANGUAGE*

Stephen Tyma
University of Michigan

"Do not be troubled by the fact that [some of the model languages introduced for didactic purposes here] consist only of imperatives. If you want to say that they are incomplete, ask yourself whether our language is complete--[and] whether it was complete before the symbolism of chemistry or the notation of the infinitesimal calculus were incorporated into it, for these are, so to speak, the suburbs of our language. (And how many houses or streets does it take before a town begins to be a town?) Our language can be seen as an old city: a maze of little streets and squares, of old and new houses, and of houses with additions from various periods; and all this surrounded by a multitude of modern sections with straight, regular streets and uniform houses."¹

0. Introduction. Wittgenstein's very appropriate metaphor for language should serve as a reminder to the linguist that it is all too easy in linguistic inquiry to favor one aspect of language to the detriment of the others. For far too long, American linguists, with the exception perhaps of Sapir, Whorf and Pike, have been too preoccupied with the structures of Wittgenstein's buildings, losing sight of the streets and neighborhoods of languages. The broad patterns of the suburbs of language have seduced too many to the conclusion that the most powerful description is the best, the effect of which is to lose sight of the smaller kernels of regularity to be found in the older quarters of the core of the city. Linguists tend to forget that not only the forms of present day utterances but the topics as well correspond to a great degree to the forms and topics of the utterances of the generations who have preceded us. That we are concerned with many of the same subjects as our forebears is a bitter pill to swallow, for it is far easier to believe on the face of the differences in appearance from one era to another that the change is more extensive than it may be.

Wittgenstein's metaphor also very aptly reminds the linguist that the connections between the roots of a language and its present state are at once indirect and recoverable, for it must be the case that the capacity to express a thought or an idea must develop along with the terms and predications for the expression of that thought or idea. Evolution is not a one-sided adjustment. The development of metallurgical technology may cause a society to abandon flint technology, but the purposes and procedures that underly both are the same. Bateson (1972b) points

out that the evolution of the horse from Eohippus was not just a horse adjusting to life on the plains, for the plains themselves evolved with respect to the evolving Eohippus. It is not the "thing" that evolves as much as it is the context. Thus it is that in Wittgenstein's city of language, calculus and chemistry developed into suburbs after somethinkers began to move out of the city.

The circumstances are such that in order to appreciate the relationships of present day English to its past, one must focus not on the things (the 'structures' or the 'buildings') that are related as much as one must focus on the relationships the contexts or the environments (the streets and squares) have with one another. It is not easy to see the patterns, for as Leach (1972) and Cunningham (1973) have shown, the patterns are often difficult to ascertain because our perceptions of certain surface characteristics are somewhat beclouded. Assuming that all human behavior including language is similarly patterned is an easy task, but 'proving' it is of an altogether different order, as Pike (1968) has shown. To borrow a Bateson simile, the linguist who accepts this challenge is like a man trapped by fire on the fiftieth floor of a burning building. The worried man can see on the fiftieth floor of the building across the street a second man who senses the predicament and shouts to the first "Jump over to me! Jump over to me! You can make it here in two jumps." In short, to establish the nature of the relation between the present state of a language and its history, linguists may see where they need to go, but they realize there is no straight, easy path to get there.

1.0 The methodology of the investigation of contextual patterns. Several assumptions must be clarified before one attempts to establish the contextual patterns of language.

1.1 Defining contextual patterns. The epistemology of generative linguistics is inappropriate to the task of discovering paradigmatic relations because of its emphasis on a rule-oriented approach. The generative school has traditionally favored maximally powerful rules to keep the components of description to manageable ("elegant") proportions. Perhaps it is this maxim more than any other that prevented Langendoen (1968) from appreciating Firth's (1957) contention that features 'discovered' by a linguist are purely formulaic descriptions of the inter-relations of the systems of a language. In addition, following Pike and Pike (1977), it is possible to posit a paradigmatic regularity that is reflected in many (if not all) levels of language. Because the structure of this paradigmatic regularity mirrors the structures of the nature of the reality indigenous to a language, information of this type is highly relevant to the gnostemic stratum in a stratificational model such as that found in Lockwood (1972). Becker (1977), Firth (1957), Geertz (1973, 1975a, 1975b), Pike (1968), Whorf (1951), Wittgenstein (1958) and others have found it useful to assume that the best mode for description of a language is that

which best captures the dimensions of reality as they are shaped by that language. This has been difficult medicine for most American linguists to take, for it is a degenerate linguistics in that it favors the language specific over the language universal. This linguistics of the particular does not exclude information of a universal nature, but likewise it does not depend on it. Rather than chasing the dream of generality, this kind of linguistics attempts to trace the beautiful, intricate web of meaning, the map of the turning, twisting streets of the older quarters of Wittgenstein's city. A language cannot be adequately described independently of information about the cultural context in which it belongs.

1.2 On discerning contextual patterns. In uncovering the paradigmatic regularity of language, it is necessary to answer questions such as those that deal with the wherewithal of the phonetic similarity of a set of words such as sperm, germ, dermis (i.e., "skin"), hermetic, fermentation, and germination. All of the words in this rather limited set contain a common -erm- formative; is this a phonetic accident? A question then arises about certain kinds of polysemy. Is it the case, for example, as Wiggins (1971) and Alston (1971) might claim, that there are two homophonous words term, with term₁ glossed as 'a period of time' and term₂ glossed as 'word' or 'name'? Are there also two words bank, with bank₁ glossed as 'rising ground bordering a river' and bank₂ glossed as 'an establishment for exchange and custody of money'? Or is it the case that the pairs term₁ and term₂ and bank₁ and bank₂ are merely twisted occurrences of some basic words term₀ and bank₀? Is each of these "pairs" a victim of phonetic coincidence like the -erm- set? Words that appear to lack some common phonetic characteristics but share others as well as vaguely similar semantic features are not immune from investigation, as in the case of pairs of seemingly related words such as discuss and discourse, defend and defer, and cuss and curse.

2.0 Etymology and the present. One of the reasons it is sometimes hard to separate the scope of historical linguistics, the scope of comparative linguistics and the scope of internal reconstruction is that there is a point at which all three converge, which is the city of language as it is now spoken, the present state of the language whose history is being discovered, whose comparison is being offered and whose internal dimensions are reconstructed. When languages are compared, questions of their convergence or divergence in time are raised. When the historical progression of a language is traced, questions about that language's siblings inevitably emerge. Finally, when the understructure or base structure of language is being secured, there arise questions about language through both time (history) and space (comparison).

To take a very crude and elementary example, it is possible to examine three English words heart, core and encourage in terms of the history of English, the comparison of English with other,

related languages, or the internal reconstruction of English. Thus, it has been attested that heart is drawn from the Latin cor ('heart'). It is also known the Romans thought the heart to be the center of the body, hence the word core. Anglo-American culture still identifies the quality of bravery with the conceptual center of the body, so 'to give someone heart' is to encourage him.

By expanding his investigation horizontally, a linguist can ascertain through the comparison of languages that the French and Spanish equivalents of English heart are cœur and corazon, respectively, or that the French encourager and the Spanish animar both correspond to the English to encourage. The rough equivalencies of English core, French coeur and Spanish cuesco can also be seen to demonstrate two of the sound shifts common to the Romance family, vowel fronting/diphthongization (o → ue) and rho degeneration (r → s).

Internal reconstruction is a horse of quite a different color. Before beginning to reconstruct English, the linguist must recognize that heart, core and encourage are all concerned with a concept of 'centeredness', 'essentiality' or perhaps even 'life' before he goes about explaining how the sound system of English adjusts to recognize the different contextual functions of this concept. At the very center of this reconstruction is a thematic regularity found in heart, core and encourage. Internal reconstruction is predicated on a thematic consistency that is vital to the historical, comparative, and internal dynamics of language. Thus, Spanish animar ('to encourage') is thematically different from English encourage and French encourager because it is based on a qualitatively different concept. For reasons that can only be founded in the premises of Iberian culture, animar is metaphorically distinct from encourage and encourager.

3.0 The pattern of language. It is necessary to directly address questions about the regularity of the -erm- set (section 1.2), polysemy (term₁ and term₂) and 'pseudo-polysemy' (discuss - discourse). However, just as the haven for the man in Bateson's building cannot be gotten to directly, so too is the path to the answers for these questions not a direct one. To liberally paraphrase Kenneth Pike, there are several mighty leaps involved. It is perhaps best to handle all the jumping through the time-honored analytical concept of abduction.

Peirce (1957) defines an abduction as the inferential step of the first starting and entertaining of an hypothesis. Where a deduction is an attempt at mathematical demonstration and induction an operation that includes the assent to a proposition put forward, abduction covers all the operations by which theories are generated. An abduction is somewhat akin to an homology in that it allows an investigator to impute similarity in observed phenomena. When Pike has imputed that human language is a behavior structured like all other forms of human behavior, he has made an abduction, for from Peirce's perspective, Pike is motivated to reduce the number of descriptive statements he has to

make about all forms of human linguistic and non-linguistic activity.

Bateson uses the term metaphor to mean abduction when he is comparing information systems. Geertz (1973, 1975a, 1975b) has discussed at length the importance of the metaphors of meaning in all aspects of society and social behavior, including but not limited to language, religion, dietary restriction and even the ill-defined concept of "common sense". Metaphors are what render behavior meaningful in that they provide frames or structures for interpretation. Metaphors inform (in the sense 'give form to') human behavior. An observer who synthesizes the metaphors that inform the behavior of a group he is investigating will begin to understand the premises or assumptions about the nature of reality as it is seen by that group. Metaphor is a means by which people orient themselves to their world. From a phenomenological perspective such as this, a culture can then be defined as a group whose members share the same presuppositions about the nature of reality. These premises differ from one culture to another, varying here greatly and there only slightly.

3.1 Metaphor and Meaning. It is possible to understand the word metaphor to mean one of two not inconsistent concepts that stand in a hierarchical relationship to one another, with one meaning subsumed by the other. One of these senses (the lower) is that familiar to most: metaphor is the perception of an event, process, object or attribute in terms of another event, process, object or attribute. Geertz sees metaphor as extending language "by broadening its semantic range, enabling it to express meanings it cannot or cannot yet express literally." Metaphor fulfills an expression function that cannot easily be handled in a literal fashion. Burke (1969) says that metaphor is "seeing the thatness of a this and the thisness of a that." Percy (1958) claims that a metaphor is actually "wrong", but somehow it is most successful when it is most mistaken. Metaphor is an abduction made in a way to be understandable and accessible for other speakers of a language.

3.2 Metaphor and reality. As Wittgenstein pointed out, the metaphors of calculus and chemistry drew upon predications for which the formal basic criteria were already present. These basic criteria describe the second sense of metaphor, which can be called root metaphor to distinguish it from literary or poetic metaphor. The root metaphor is constructed by an observer from without who places himself within. Pike, in effect, says a linguist describing a language constructs root metaphors:

"The understanding of a language involves the observer-linking [i.e. the linking by an observer] of a linguistic form to an experienced meaning. This form-meaning integration takes place relative to a universe of discourse, a generalized field of observation, cut out by the observer from possible areas of interest to him." ²

Root metaphors thus give the actual dimensions and shape of the

presuppositions of the culture whose language is being analyzed and described.

This fusion of form ("sound") and meaning is central to Firth's (1957) analytical concepts of prosody and phonematic unit. A prosody suggests a common theme and is merged with a phonematic unit, which suggests another theme. The union of the two themes results in a structural relationship or root metaphor. The triliteral root n-s-b in Moroccan Arabic is a rough example of this merger. There are many words in Moroccan Arabic that contain the complex n-s-b, and generally all these words convey a sense of "attribution", "connection" or "affinity". Nsīb means "in-law"; nsab means "to attribute or impute to"; munasāba means "a relation", "an analogy", "a correspondence"; passab means "genealogist"; nisbiya means "(physical) relativity"; and so on for several more derivatives. There is a metaphor of "attribution", "imputation" or "relation" that is common to all these words. To somewhat distort Firth's original sense of the term, a prosody n-s-b marks a common theme in all of these Arabic words (Geertz 1975b).

This same sort of thematic index is present in the -erm- set discussed in section 1.2, but similarities of this sort are rarely, if ever, alluded to in dictionaries of English. Sperm and germ, as elements of nature, are seen to share some attributes in that they are made somehow to enter an organism and, under the proper conditions, incubate, causing what is seen as a non-normal state of pregnancy or disease. Fermentation is a process somewhat similar to pregnancy or disease in that it results from the introduction of some form of bacteria to sugar, the result of which is the manufacture of alcohol and carbon dioxide. The words dermis (skin) and hermetic suggest impermeability or integrity: the dermis is responsible for protecting and containing the body, and something that is hermetically sealed cannot be penetrated by micro-organisms. All the words in this -erm- set share a semantic characteristic or theme in that they are concerned either with agencies of change or resistance to such agencies. Most dictionaries fail to capitalize on this kind of thematic regularity.

This particular rho prosody³ [(V)Vr(C)] conveys different dimensions of a root metaphor that are intimately intertwined. One such dimension is that of "agency" or "instigation", as is found in words like spark, sperm, germ, virus, energy, and perhaps even warm or heart (see Appendix A). Another dimension of the root metaphor captured by the rho prosody is a sense of process or change of state as found in words like burn, ferment, germinate, history or preserve (see Appendix B). These processes or changes of state often have agents (instigators) that are marked with rho prosodies (e.g. spark and burn are related to one another, as are bacteria and ferment). A third dimension of the root metaphor is one of "integrity", "unit", or even "protection" found in words like dermis, hermetic, guard and term (i.e. "period of time"). (See Appendix C.)

Observing that term conveys a sense of "unit" helps resolve

the dilemma of dealing with the polysemy in term₁ ('period of time') and term₂ ('word or name') raised in Section 1.2. Term₁ is generally used to refer to a period (or "unit") of time the beginning, duration and end of which are reasonably certain, as in school term or a term of office. Term₂ can then be seen as denoting a conceptual unit, separated, as it were, from the stream of conceptualization. A term denotes a particular concept, somehow distinct from all other concepts. Thus, term₁ and term₂ are informed by the same root metaphor (term₀); the different "senses" of term₁ and term₂ derive from the contexts in which they are used. The same is true of bank₁ and bank₂. It seems hardly accidental that just as riverbanks (bank₁) constrain water, money banks (bank₂) constrain money or that a quality of motion is present in bank₁ as current and in bank₂ as currency. Banks₁ direct the flow of water and banks₂ direct the cash flow. Taken to the level of root metaphor constructed by the observer, term₀ and bank₀ capture structures that are recontextualized appropriately to the particulars of context.

There appears to be yet a fourth dimension to the root metaphor represented by the rho prosody, and this dimension signifies a kind of hierarchy. For example, the word world indicates a hierarchy in that worlds are themselves composite entities (i.e., composed of 'different' things such as cultures, mountains, gases, etc.) and yet worlds together form a complex entity like solar system or universe. An organ is of much the same order in that it is composed of pieces (e.g. enzymes, lobes, chambers) to effect a process (e.g. digestion), yet it is itself a piece (an 'agent') in a larger structure (i.e. a body). Other examples of this kind of hierarchy are given in Appendix D.

It must be understood that this kind of thematic analysis is not merely sloppy morphology. If all the terms presented here are analyzed in a more traditional (i.e. 'generative' or 'structuralist') mode, the sense of paradigmatic regularity is either lost or sufficiently obscured. The pseudo-polysemic relation between words like discourse and discuss cannot be handled in a programmatic framework. (What is the rewrite rule for discourse → discuss?) The rho prosody signifies important differences in known dimension. A discourse generally has a known beginning, duration and end even when it is a piece of a larger universe, while a discussion is less directed. To discourse on something is to lead someone to a known objective, but to discuss is to seek to formulate an objective or resolution. Similarly, one can defer something the dimensions of which are known or thought to be known (e.g. 'deferring a problem until later'), but generally one defends against that which does not have known dimensions.

3.3 Restricting the range of metaphor. As Firth himself has said, it is not appropriate to collapse all incidences of a phonetic feature into the same prosodic formula, a claim that bothered Langendoen in his critique of London School linguistics (1968). (Perhaps Langendoen's inability to understand this stems

from the considerable epistemological differences between generative linguistics and London School linguistics.) For example, it would appear to be wrong to attempt to consider the -er, -re of the comparative degree of English modifiers (e.g. bigger, other, more quickly) because even though a rho element is present in these words, they do not share the same root metaphor as sperm, fermentation, term, or organ.

4.0 The role of metaphor in relating synchrony and diachrony. All the foregoing does pertain to the relation between synchrony and diachrony in the analysis and description of language. The linguist must remember, as did Benveniste (1969), that when people use language, generally speaking, they do so to talk about something that is relevant to them or at least to try to find something that is relevant to them. As the etymology of relevant suggests, that which is relevant is also somehow real. That which is real to someone is grounded in his presuppositions about the nature of reality.

To relate the present state of a language to its former states, a comparison of that language's metaphoric changes through time must be undertaken. As Geertz (1973) has pointed out, metaphors are much more resistant to change than thought by those more mechanistically oriented, who have been seduced by the thought that changes in the hardware of culture always reflect changes in the software (cf. Preziosi 1976). Those so seduced are merely allowing the epistemology of their own culture to interfere in their perceptions of the patterns of human behavior. One of the most glaring shortcomings of modern language teaching pedagogy is that it fails to provide as much insight to the metaphoric structure of language as it could.⁴ Clarke's conception of the clash of consciousness in the learning of a second language, the double bind of knowing that your reality is right and discovering that someone else's reality is right too, yearns to be explored in the light of metaphoric analysis.

A good example of metaphoric change through time can be found through examining the Anglo-American conceptualizations of fermentation and germination. In a highly technical manner of speaking, it is correct to say that the causative agents of both of these processes are bacteria similar to germs and viruses. However, the more common name for 'agent of fermentation' is yeast. The Oxford English Dictionary has documented that yeast and germ have a common ancestor in /germ/. In addition, the word kernel once meant roughly the same as the present word seed, the stuff which is germinated. These changes in the form of /germ/ and the meaning of kernel are apparently of some significance. Perhaps the changes have been motivated by changes in metaphor. Yeasts and seeds are not really agents like germs, for germs and the like are subject to a lesser degree of control than are yeasts and seeds. Instead, yeasts and seeds are subject to a high degree of control. Metaphorically, yeasts and seeds are instruments to effect fermentation and germination.

5.0 Conclusion. The claim that English has more than 600,000 words is not really accurate for two reasons. First, many artificial divisions have been made, such as those in the cases of 'polysemic' term and bank. Secondly, an analysis and description of English framed in the abductions used here will begin to resemble the more 'traditional' descriptions of languages like Arabic (cf. Mitchell 1960) in that they will tease out the features like the rho of 'delimitation'. Prosodic descriptions will probably not be more 'powerful' or more 'economical', but they will be more aesthetically pleasing in that they will more accurately describe the streets and the architecture of the buildings in Wittgenstein's city. As Sapir (1949) has suggested, linguists must begin to appreciate the unconscious patterning of language that is indicative of the many different ways in which people see themselves and the world around them.

Notes

*I wish to acknowledge the many discussions with Profs. A.L. Becker, Vern Carroll and Horacio Fábrega from which I have benefited greatly in the preparation of this paper.

¹ Wittgenstein (1958), p. 8. I have taken the liberty of altering the Anscombe translation in the manner of Geertz (1975a).

² Pike (1964), p. 130.

³ I do not intend for this term to be understood in exactly the same sense Firth intended. Instead, I intend the term to mean "phonetic-semantic complex", like Pike's form-meaning composite. If it is at all disconcerting that I misuse the term prosody, I am amenable to having the terms phonestheme or phonosememe substituted.

⁴ For a more detailed criticism of this sort, see Tyma (1976).

Appendices

Appendix A: Examples of agent terms

spark	agent of ignition
energy	agent of motion
sperm	agent of (human) fertilization
germ	agent of biomedical disease
virus	agent of biomedical disease
heart	agent of blood flow maintenance
arm	agent of activity (human directed)

Appendix B: Examples of process or change-of-state terms

burn	process of incineration (oxidation)
earn	process of acquisition through exchange

learn	process of acquisition of knowledge
turn	process of directional change
pare	process of change of form (achieved by cutting)
cure	process of returning to healthy state
history	process of (documentation of) evolution of human activity
ferment	process of manufacturing alcohol from sugar

Appendix C: Examples of integrity index terms

term	index of finiteness
firm	index of macroscopic integrity
hermetic	index of microscopic integrity (impermeability)
dermis	index of human organic pseudo-integrity
border	index of spatial delineation
far, there	index of remote integrity
here	index of local integrity
era	index of time unit

Appendix D: Examples of hierarchy terms

	Process (level 1)	Agent (level 2)
organ	collection of subfunctions	part of bodily ecology
world	collection of systems	member of system
organisation	system of internal dependencies	member of macrosystem

References

- Alston, W. 1971. How does one tell whether a word has one, several, or many senses? In D. Steinberg and L. Jacobovits (eds.), *Semantics*. London: Cambridge University.
- Bateson, Gregory. 1972a. *Steps to an Ecology of Mind*. New York: Ballantine Books.
- _____. 1972b. Comment on part II of 'Steps to an Ecology of Mind'. In Bateson, 1972a, pp. 153-156.
- _____. 1972c. Style, grace and information in primitive art. In Bateson 1972a, pp. 128-152.
- Becker, A.L. 1977 (in press). Text-building, epistemology and aesthetics in Javanese shadow theatre. In A. Becker and A. Yengoyan (eds.), *The Imagination of Reality*. Ann Arbor: Association for Asian Studies.
- Benveniste, E. 1969. *Le Vocabulaire des institutions Indo-Europeennes*. Paris: Editions de minuit.
- Burke, K. 1969. *A Grammar of Motives*. Berkeley and Los Angeles: University of California.
- Clarke, M. 1976. Second language acquisition as a clash of consciousness. *Language Learning* 26:377-390.
- Cunningham, C. 1973. Order in the Atoni house. In R. Needham (ed.), *Right and Left: Essays on Dual Symbolic Classification*. Chicago: University of Chicago.
- Firth, J. 1957. *Papers in Linguistics, 1934-1951*. London:

- Oxford University.
- Geertz, C. 1973. *The Interpretation of Cultures*. New York: Basic Books.
- _____. 1975a. Common Sense as a cultural system. *Antioch Review* 33.5-52.
- _____. 1975b. On the nature of anthropological understanding. *American Scientist* 63:47-53.
- Langendoen, D. 1968. *The London School of Linguistics: A Study of the Linguistic Theories of B. Malinowski and J.R. Firth*. Cambridge: M.I.T.
- Leach, E. 1972. Anthropological aspects of language. In D. Miranda (ed.), *Mythology*. Baltimore: Penguin.
- Lockwood, D. 1972. *Introduction to Stratificational Linguistics*. New York: Harcourt, Brace, Jovanovich.
- Mitchell, T.F. 1960. Prominence and syllabification in Arabic. *Bulletin of the School of Oriental and African Studies* 23.369-389.
- Peirce, C. 1957. *Essays in the Philosophy of Science* (V. Tomas, ed.). Indianapolis: Bobbs-Merrill.
- Percy, W. 1958. Metaphor as mistake. *The Sewanee Review* 66: 79-99.
- Pike, K.L. 1964. Beyond the sentence. *Journal of the Conference on College Composition and Communication* 15.129-135.
- _____. 1968. Language in Relation to a Unified Theory of the Structure of Human Behavior. The Hague: Mouton.
- _____. 1975. On describing languages. Lisse: The Peter De Ridder Press (also appears in Austerlitz (ed.), *The Scope of American Linguistics*, p. 9-39).
- Pike, E. and K. Pike. 1977 (in press). *Grammatical Analysis*. Huntington Beach, California: Summer Institute of Linguistics.
- Preziosi, D. 1976. Toward a relational theory of culture. In R. DiPietro (ed.), *Proceedings from the Third LACUS Forum*. Columbia: Hornbeam.
- Sapir, E. 1949. *Selected Writings in Language, Culture and Personality*. (D. Mandelbaum, ed.) Berkeley and Los Angeles: University of California.
- Tyma, S. 1976. Review of *Teaching English as a Second Language: Techniques and Procedures*, by C. Paulston and M.N. Bruder. *Language Learning* 26.433-442.
- Wiggins, D. 1971. On sentence-sense, word-sense and difference of word sense. Towards a philosophical theory of dictionaries. In D. Steinberg and L. Jacobovits (eds.), *Semantics*. London: Cambridge University.
- Witherspoon, G. 1977. *Language and Art in the Navajo Universe*. Ann Arbor: University of Michigan.
- Wittgenstein, L. 1958. *Philosophical Investigations* (3rd edition). Translated by G.E.M. Anscombe. New York: Macmillan.

LISLAKH

Carleton T. Hodge

Indiana University

I find I cannot with honour shrink from some sort of comparison of my Egyptian forms and roots with the Semitic and Iranian forms and roots. The facts are so enormously great, that it does not in the least matter whether the proof can be *thoroughly* given in all its details (Bunsen 1851 *apud* Müller 1880.3: 429).

Were Egypt but firmly established as the primitive Asiatic settlement of the as yet undivided Arian and Semitic families, we should have won the game for the recognition of historical truth (Bunsen 1854 *apud* Müller 1880.3: 464).

These two quotations may well deserve the honor and attention paid to the famous statement of Sir William Jones with regard to those languages later to be known as Indo-European (IE). Should we not add Christian Karl Josias, Baron von Bunsen, to our histories of linguistics as one of the great perceptive minds, whose statements clearly foreshadowed the results of future research? Has he not correctly stated that Lisramic (L, Egyptian [Eg.] and Semitic [Sem.] being representatives) and Indo-Hittite (IH, to him Arian) are related, and that they have their origin in Egypt (cf. below and Bender 1975: 218-25)?

Despite the lapse of over 120 years, his time has not yet come. The research which would unequivocally connect these two groups is yet to be done. Indeed, there has been over 150 years of work, but there is as yet no general recognition of the common origin of L and IH. Many people believe that they were once one. One may cite works from Lepsius (1836) to Davidowitz (p.c.), comparing the two, but the scholarly world remains unconvinced. Being one of the 'believers', I have called this putative common ancestor Lislakh (LL, Hodge 1972).

We should, however, be well aware of the nature of the problem facing us. If we are to hypothesize that LL existed, we should be ready to assign it to a reasonable place in time and space. It is by considering this task that we are led to a more accurate appraisal of the nature of this linguistic hypothesis and may plan our strategy for testing it.

Swadesh suggests that a lexicostatistic comparison may give a rough time depth of separation, which date (or dates) can then be used as a guide when one is looking at the archaeological evidence (1967: 80). In the present case we are far from such a primitive state of affairs. IE (and IH) studies, including speculation on

the homeland and the archaeology thereof, have been with us for 10 these many years. L, while not as well investigated, has a long if not as respectable a history. While we are in a position to consider a certain amount of evidence from both sources, it would appear that the archaeological data are presently more revealing than the linguistic.

No such heresy as the identity of language and culture is here proposed or even implied. One does assume, however, that groups as large as the proto-IH (pIH) and proto-L (pL) ones must have been would have left material evidence of their existence, and that such evidence is either known or recoverable.

Gimbutas has plausibly suggested that the common IE (here = IH) period is to be associated with the archaeological grouping named by her Kurgan ('barrow', Gimbutas 1970). Discussion of this (and other) archaeological remains associated with IH peoples are often (and understandably) concentrated on the 'leading edge' of such prehistory, the archaeological complexes immediately prior to and associated with the emergence of the various IH peoples into history (e.g. Crossland 1971). Interest in the homeland may likewise concentrate on it as a point of dispersal. If we are to be successful in our quest, we must rather be concerned with the earliest date such a language-related complex is identifiable, where it is located and what possible connections it may have had with preceding ones. In this case Gimbutas informs us that the Kurgan culture itself is known as far back as the fifth millenium B.C. and 'most probably developed from a Mesolithic group in a limited geographical area between the Don River and the southern Urals' (Gimbutas 1970: 190). Goodenough, in the same volume, argues for a date of origin of the Kurgan cultures about 3500 B.C., with connections to the west (1970: 260, 258). He also notes that the 'neolithic economy, with mixed agriculture and animal husbandry, is now known to have come into eastern Europe from Anatolia as early as 6000 B.C.' (258). (It is not our purpose to mediate these variant views but to utilize them as best we may in the linguistic context.)

Linguists generally put the date of pIH (pIE) before 3000 B.C. (e.g. Lockwood 1972: 1). Lehmann attributes even this time depth to the addition of Anatolian: 'With Hittite we are in a position to reconstruct Proto-Indo-European of a period before 3000 B.C.', adding, very curiously, 'This date nullifies older attempts to relate Indo-European with other language groups' (1962: 40). Ruhlen dates pIE (pIH) about 4000 B.C., beginning to break up by 3000 B.C. (1976: 67). Swadesh, on the other hand, says that the proto-language 'may go back at least seven thousand years before the present' (1971: 84; cf. his divergence of 69 minimum centuries for IE [IH], 1967: 88). As he has taken archaeological evidence into account, the agreement with Gimbutas is not surprising, though one may reach such time depths even on strict lexicostatistic grounds.

Using Gimbutas and Swadesh as mutual props for the earlier date, we may tentatively put pIH in the steppe area some 4500-5000 B.C., keeping in mind that no clear archaeological origin is known for this group.

For L the archaeological evidence, as interpreted by Munson (forthcoming), is striking and very revealing vis-à-vis our problem. About 18,000 B.C. we find a group of related cultural complexes in what is now southern Egypt and northern Sudan. A variety of this, Oranian, was established along the Mediterranean coast some 14,000-12,000 B.C. Another branch appeared by 9000 B.C. in western sub-saharan Africa. Somewhere between 15,000 and 10,000 B.C. a related group is found in the lake region of Uganda and northwestern Kenya. It is reasonable to assume that we have here at 18,000 B.C. the homeland of the L languages and in the other, related culture, evidence of the split into Berber, Chadic and Cushitic and/or Omotic (15-9,000 B.C.). Travel down river (evidence later - ca. 5,000 B.C.) gives us Egyptian. (The preceding is based entirely on Munson, forthcoming, q.v.). Bender came to much the same conclusion regarding the L homeland, but without the dates and details Munson is able to furnish (cf. Bender's map, 1975: 225). We may also note that this view comes close to confirming the L part of the second Bunsen quote above.

Applying the above data to the problem of relating IH and L, we are struck by the disparities in space and time. While it is not too far from the valley of the Nile to the steppe area (Kurganistan, may we say?), we need archaeological evidence that there was a connection between the two. As we have seen, Gimbutas tends to connect the Kurgan culture with the northeast, Goodenough with the west. If LL proves linguistically viable, we must find the southern connection.

The dates of the material cultures discussed are not precise, but they are more to be relied upon than the rough approximations of linguists or even those of lexicostatistics. We have now a more accurate concept of the temporal aspects of our comparison. These are charted in terms of millennia B.P. in Table 1. If we accept the upper limit of divergence, pL begins to break up about 17,000 B.P. The lower limit is about 14,000 B.P. If we take the latter as more probable, a connection between IH and L would have to antedate the first divergence from L, about 14,000 B.P. Turning to Swadesh's estimates, we find that he assumes that 'Hamitan' (L) and IE (IH) are related but are separated by at least 67 centuries. To this we must add his minimum centuries divergence for IE (IH), namely 69 (1967: 88). The connection between the two is therefore at least 13,600 years B.P., a remarkable correspondence with Table 1. Swadesh gives no minimum divergence for his Hamitan (L).

The present discussion has assumed that all branches (or language families) now considered to belong to L are related to each other more closely than any one of them is to IH. If this assumption is wrong, there is, of course, no necessity for dating pLL prior to pL; IH becomes part of the group and fits in somewhere down the time scale. While such speculation is legitimate and desirable, there seems to be little basis for it. Omotic appears to be the most divergent of the L languages (Bender 1975). Bender's grammatical and lexicostatistic study of the place of Omotic in L includes reconstructed pIE (not pIH) forms for comparison. Cognate percentages of Omotic with the other branches do not appreciably

differ from those of Omotic with IE (Bender 1975: 216). However, even brief examination of Omotic basic vocabulary in Fleming (1976: 316-21) shows definite ties with other L rather than with IH. It is more reasonable, linguistically (and archaeologically) to assume that IH, if related, must go back to a proto-language antedating the earliest split from pL. At the same time one must emphasize that the relationships of the L languages to each other are still far from clear. One may also add that the apparent discrepancies between the archaeologically attested sequence of L splits and those postulated by Bender on linguistic grounds are not at present resolvable (cf. Bender 1975: 224).

The implications of Table 1 both for the study of L and for the LL hypothesis are far-reaching. For L alone we are dealing with a time depth triple that of IH. We are, in fact, pressing at the limits of what present techniques can do for us. 'There can be no doubt that there are already successful probings of time-depths far greater than 5000 years, possibly up to 15,000, and that even more can be achieved by a systematic and careful application of already known techniques' (Swadesh 1959: 27).

Our archaeological and temporal disquisition has presented us with a framework to which we may direct our methodology, our 'already known techniques'. But given a time depth of over 15,000 years, what tools have we for establishing genetic relationship at this distance? There is as yet no full answer. We must devise techniques as we go along, but what has been done to date does give us some useful leads. The comparative study of L has always assumed that both lexical and morphological elements are found in common among sufficient of the branches to assign them to the proto-language (cf. Bender 1975 for both history and a current effort). Is there any indication as to which of these has the greater time depth, i.e., may be expected to survive in an identifiable form longer?

In the First LACUS Forum I presented a set of postulates for comparative linguistics. It is therein stated: 'Morphs having form/meaning similarity are at present those items of structure which appear to be identifiably derivative through time for the longest periods' (Hodge 1975b: 211; 'similarity' refers to similarity of morphs at various points in time). This is a bald statement to the effect that the lexical item has the temporal edge in this respect. One could quote opinion from Thomas Young (Wells 1973) to Morris Swadesh in support of this, and just as many, if not more, *contra*. The claim was based on none of these but on a historical study of the survival of ancient Egyptian elements into Coptic (Hodge 1975a). Analyzing a group of very ordinary Coptic sentences (classroom expressions, I take them to be), the paper traced each morph in them, and each morphologic construction, back where possible to the older language. The results showed that the morphs themselves, not combinations of them, appeared traceable for the longest period of time. It was also clear that certain syntactic structures and even some morphology were recognizably the same after 4500 years. Morphology as a whole changed drastically. The syntactic survivals were so general that it is not presently advis-

able to use them as relational material. It is to be stressed that the Egyptian survival study was a historical one, involving no reconstruction or consideration of prehistory. Within a single language we can attest the greater survival of lexicon than of morphology.

It is therefore concluded that a lexical approach would be the most apt to yield usable results. It is natural in such a case to turn to current usage of core vocabulary in making such a comparison. Assuming that pLL dates to 16,000 B.P., what percentage of lexical survival could be expected? Less than one percent, according to Gudschinsky (1956: 207), some four percent according to Swadesh (1971: 284 [originally 1960]). Such a small number of cognates would not furnish much in the way of sound correspondences. The situation is similar to that met in the study of certain Central American languages: 'However, such preliminary comparisons left open doubts due to uncertain cognate identification. This called for careful study of phonologic agreements covering not only the words contained in the 100-item diagnostic list but also a sufficient amount of additional material to establish or at least make probable the historic reality of the assumed phonemic developments' (1967: 87). Our first task is, therefore, to broaden the base of comparison. 'Additional material' must be found sufficient to establish the sound correspondences needed to assure genetic relationship. We must not, however, revert to type and do either mass or random comparisons in order to come up with the necessary lexical quota. We would only defeat our purpose, as so many have before us. The core vocabulary must be expanded in such a way that it remains basic.

Before proceeding with this, we should call attention to another, extremely important, aspect of the comparison. We have in IH and L not only languages with long, well attested histories; we have to a certain extent reconstructed proto-forms in each camp. Thousands exist for pIE, fewer and less certain ones for pIH. We have as yet few reconstructions attributable to pL, but there are many for pSemitic, a considerable number for pCushitic, fewer for pChadic and pBerber. Egyptian, a single language, offers us attested forms older than many reconstructed ones. Ideally we should wait until we have a full set of reconstructed pL forms, but a preliminary comparison of what we now have should at least tell us whether we are on the right track.

The use of reconstructed forms also enables us to cut the time differential between pIH and pL by 4-5,000 years, increasing the chances of cognate occurrence, though not, it would appear, a great deal. There is, however, another aspect of these forms which will materially aid us in broadening the vocabulary base. Apart from such classic examples as the reconstructability of the proto-Algonquian word for 'whiskey', the very fact that an item is reconstructable is evidence of its centrality to the proto-culture. Given the geographical separation of our two groups, we can be further selective in our choice of items, choosing ones which are likely to occur in both places. Archaeology is again helpful, reminding us that we should not expect agricultural or animal husbandry terms at such an early period.

It is therefore proposed to build a sequence of core vocabularies, the comparison to proceed from one to the next, building on the presumed cognates in the first to control correspondences in the second, and so *seriatim*. The initial list would be a modified Swadesh 200 with additions from reconstructed etyma. The second would add other basic words found to be in common to all or most of the reconstructed vocabularies. A further expansion would broaden the base by the use of actual language forms, as well as reconstructed ones, not in the first two lists but semantically related to them.

The last, which could be handled by semantic domains, semantic features, or both, would enable one to recover such (presumed) relations as that of Eg. *db* 'hippopotamus' to Hausa *dooki* 'horse'. Obviously pL had no horses, nor had pIH any hippopotami. The semantic ranges of known etymologies are so great that this aspect of the comparison would have to be carefully controlled and use of it made only after sufficient sound correspondences had been established on other grounds.

Only in such a way can we hope to set up sound correspondences which will be convincing to the linguistic community at large and provide the basis for a broad comparison of vocabulary which would clarify the relationship by providing greater detail.

The reader may have noticed that I have ignored practically all previous work which purports to relate IH and L, including my own (published and unpublished). It is almost pointless to discuss it until such time as the proposed project is completed, or at least well under way. We may then confirm or set aside previously offered etymologies and assess the insight of the investigators, lavishing praise and politely withholding blame. But without following strict procedures we cannot give them their just desserts.

TABLE 1

B. P.	IH	L					
20000		pL?					
19000							
18000							
17000						?	
16000				?			
15000		pL?					
14000				pBer			
13000							
12000					pCush/0m		?
11000						pChad	
10000							
9000							
8000							
7000	pIH			pEg?			
6000		pSem?					
5000		X	X				
4000	X	X	X				
3000	X	X	X				
2000	X	X	X	X			
1000	X	X	X	X	X	X	

Notes: ? in column indicates upper limit of divergence.

X indicates attestation within a millenium (fragmentary occurrence, such as Berber in Egyptian, not noted).

REFERENCES

- Bender, M. Lionel. 1975. *Omotic: A New Afroasiatic Language Family*. (University Museum Studies. Research Records, 3). Carbondale: University Museum.
- Crossland, R.A. 1971. "Immigrants from the North." *The Cambridge Ancient History* 1.2: *Early History of the Middle East*, ed. by I.E.S. Edwards, C.J. Gadd and N.G.L. Hammond, pp. 824-76. Cambridge: At the University Press.
- Davidowitz, Gilbert. 1977. "Synopsis of Some Cognate Sets in Hamito-Semitic--Indo-European." (MS)
- Fleming, Harold C. 1976. "Omotic Overview." *The Non-Semitic Languages of Ethiopia*, ed. by M. Lionel Bender, pp. 299-323. (Committee on Ethiopian Studies, Occasional Papers, 5). East Lansing: Michigan State University.
- Gimbutas, Marija. 1970. "Proto-Indo-European Culture: The Kurgan Culture during the Fifth, Fourth, and Third Millenium B.C." *Indo-European and Indo-Europeans*, ed. by G. Cardona, H.M. Hoenigswald and A. Senn. Philadelphia: University of Pennsylvania Press.
- Goodenough, Ward H. 1970. "The Evolution of Pastoralism and Indo-European Origins." *Indo-European and Indo-Europeans*, ed. by G. Cardona, H.M. Hoenigswald and A. Senn, p. 253-65. Philadelphia: University of Pennsylvania Press.
- Gudschinsky, Sarah C. 1956. "The ABC's of Lexicostatistics (Glottochronology)." *Word* 12: 175-210.
- Hodge, Carleton T. 1972. "Lisramic." *Language Sciences* 20: 13-16.
- . 1975a. "Egyptian and Survival." *Hamito-Semitic*, ed. by James and Theodora Bynon, pp. 171089. (Janua Linguarum, S. P., 200). The Hague: Mouton.
- . 1975b. "A Set of Postulates for Comparative Linguistics." *The First LACUS Forum 1974*, ed. by Adam Makkai and Valerie Becker Makkai, pp. 209-16. Columbia: Hornbean Press.
- Lehmann, Winfred P. 1962. *Historical Linguistics: an Introduction*. New York: Holt, Rinehart and Winston.
- Lepsius, Richard. 1838. *Zwei Sprachvergleichende Abhandlungen*. Berlin: Ferdinand Dümmler.
- Lockwood, W.B. 1972. *A Panorama of Indo-European Languages*. (Hutchinson University Library.) London: Hutchinson.
- Müller, F. Max. 1880. *Chips from a German Workshop*, vol. 3. London: Longmans, Green.
- Munson, Patrick H. Forthcoming. "Africa's Prehistoric Past." *Africa*, ed. by Phyllis M. Martin and Patrick O'Meara. Bloomington: Indiana University Press.

- Ruhlen, Merritt. 1976. *A Guide to the Languages of the World*. Stanford: Language Universals Project.
- Swadesh, Morris. 1959. "Linguistics as an Instrument of Prehistory." *SJA* 15: 20-35.
- _____. 1967. "Lexicostatistic Classification." *Handbook of Middle American Indians*, Vol 5: *Linguistics*, ed. by Norman A. McQuown, pp. 79-115. Austin: University of Texas Press.
- _____. 1971. *The Origin and Diversification of Language*, ed. by Joel Sherzer. Chicago/New York: Aldine-Atherton.
- Wells, Rulon. 1973. "Lexicostatistics in the Regency Period." *Lexicostatistics in Genetic Linguistics*, ed. by Isidore Dyen, pp. 118-21. (*Janua Linguarum*, S.M.. 69.) The Hague: Mouton.

AELFRIC AND BILINGUALISM IN ANGLO-SAXON ENGLAND

Connie C. Eble

University of North Carolina at Chapel Hill

It is ironic that the person who is the single best source of the English language of a thousand years ago was a Benedictine monk who chose to live his life under the rule of silence. Aelfric (c.955-1010) was the foremost writer in an era in which the prestige language Latin and the vernacular were both used for literary composition. For the first time in Western Europe there existed a thriving bi-literate culture, and Aelfric -- prolific author of liturgical homilies, Biblical translations and explications, pastoral letters, and grammatical writings -- was its finest representative.

It is also ironic that such bi-literate activity emanated from the monasteries, places which we stereotype today as emphasizing personal meditation and remote from the "real world." But the Benedictine revival and reform of monasteries in England, brought from the continent in the mid-tenth century, put the main focus of monastic life not on individual meditation but on the communal worship specified for various hours of the day and night by the Rule of St. Benedict. The emphasis was on observing the Rule and faithfully performing assigned duties. Aelfric was the product of the Benedictine revival. He saw his duty as providing the means for instructing the laity in matters of faith and piety. It was Aelfric's devotion to Christian duty as shaped by his interpretation of monasticism that led to his literary mastery of both Latin and English.

It is not difficult to account for the Latin half of Anglo-Saxon tradition. During the Middle Ages, Latin was the written language of Europe -- the language of church and state documents and virtually all intellectual activity. In an earlier age in England it had been the language of Bede, Alcuin, and the magnificently illuminated Lindisfarne Gospels. In the Benedictine monasteries of Aelfric's time, Latin was the language of most communal activities. Seven times a day the monks gathered to recite and chant prescribed Latin prayers.¹ They were encouraged to do spiritual reading, and the Rule provided that if for some reason a monk would not or could not be taught to read he should be put to some other constructive work.² During meals the monks were read holy texts by a lector chosen for his ability to sing and read.³ A brother could be severely disciplined for introducing error into a text by misreading in church.⁴ Boys in the monastery school learned correct enunciation and pronunciation under threat of whipping.⁵ And Latin was, of course, the language of the Eucharistic celebration and the sacraments.

Throughout the period Latin was the dominant literary language. Of the almost two hundred extant literary manuscripts containing substantial amounts of English and written before 1200, more than half are Latin manuscripts with English notes or glosses.⁶

The second half of the tradition, the rise of literary English, is not so easy to account for. Most Old English writings, including the four poetic codices, survive in late tenth and eleventh century manuscripts written in a more or less standardized form of Late West Saxon. King Alfred, who ruled 871-899, is often credited with beginning this standard written English. The remarkable warrior-king who saved England from invading Danes sought to restore learning to his kingdom by founding a school for the sons of freemen and by having several important medieval works translated into English and circulated for copying. But Helmut Gneuss argues that the linguistic evidence does not reveal a uniformity of dialect among the compositions associated with Alfred or a continuity between these and compositions of the late Old English period. Instead Gneuss finds the origin of standard Old English in the Benedictine revival, specifically in Aethelwold's school in the monastery at Winchester.⁷

Regardless of the specific origin of literary English, "the fifty years between 970 and 1020 saw an output of native prose and verse which alike in quality and quantity was unsurpassed for more than three centuries, and during that time the English language attained a dignity and beauty which no other native European tongue could attempt to rival before the age of Francis of Assisi."⁸ Although the bulk of Anglo-Saxon poetry was composed much earlier, *Beowulf* and the other important Old English poems exist for us today in manuscripts produced in this period of appreciation of the vernacular which came with the Benedictine revival. However, the creative energy of the revival found its expression not in poetry, but in prose.

English prose was used for explications and translations of the Bible, homilies, saints' lives, history, geography, philosophy, science and medicine, grammar, correspondence, and fictional and apocryphal narrative. These works were surprisingly sophisticated. They are not the child-like attempts of untutored writers recording themselves for the first time. Nor do they appear to be the attempts of writers who sought the comfort of their own language because they were unable to write in Latin. Although much of the prose was written for the uneducated, it was not written by the uneducated. As Peter Clemoes observes, "...late Old English prose has the marks of educational training stamped on it."⁹ Clemoes further specifies:

Education gave Old English prose writers a conscious sense of the difference between one language and another, of the process involved in transferring thought from one language to another, of the process involved in using language to communicate understanding, and of the aptness, the fitness of style. More particularly it taught them to apply a number

of expository and rhetorical techniques. They understood and used, for example, the exegetical form of the 'continuous gloss' and various types of the 'question and answer' device. They had a range of rhetorical figures at their command.¹⁰

That there was a sense of the independent character of the two languages is shown by the consistent practice of maintaining separate scripts for the recording of the two languages. The vernacular was written with the insular script developed in England, and the Latin was written with the Caroline script from the continent. In bilingual texts, including some of the manuscripts of Aelfric's Grammar, the careful distinction is readily observable.¹¹

Other evidence of the attention given to the vernacular is the quality of the manuscripts, many of which show themselves to be expensive productions by their size, generous margins and spacing, rubrication, and illustration. A dramatic example is the Old English prose version of the Hexateuch as preserved in British Museum Cotton Claudius B iv. This most extensive vernacular rendering of the Old Testament before the Wycliffe Bible of the fourteenth century contains on about 155 folios 394 pictures.¹²

Aelfric wrote in both languages, mostly in English, with sources mainly in Latin. Among his English works are 121 homilies for specified Sundays and holy days; twenty-four narrative pieces for pious reading; translations or summaries of seven books of the Old Testament; an astronomical treatise based on Bede (*De Temporibus Anni*); a grammar of Latin with an accompanying glossary; and pastoral letters. Aelfric's Latin works include the instructional *Colloquy* and *Letter to the Monks of Eynsham* and a *Life of St. Aethelwold*. Works that survive in both English and Latin versions are two letters for Archbishop Wulfstan and the prefaces to the two series of *Catholic Homilies*, *Lives of the Saints*, *Grammar*, and the translation of *Genesis*.¹³

Aelfric's choice of English rather than Latin for most of his work was a practical one: most laymen and many religious did not know Latin. In the prefaces to his initial set of homilies he first gives his reasons for writing in English.

However rashly or presumptuously undertaken, I have nevertheless formed this book out of Latin writers and from Holy Scripture, translating into our ordinary speech, for the edification of the simple, who know only this language both for reading and hearing; and for that reason I have used no difficult words, but only plain English; so that our message might the more readily reach the hearts of those who read and hear, to the profit of the souls of those who cannot be taught in any other tongue than that to which they were born.¹⁴

In a pastoral letter for Wulfsgige, Bishop of Sherborne, Aelfric advises that English be used for preaching to the laity: "The masspriest on Sundays and holy days should explain the meaning

of the Gospel to the people in English."¹⁵

In his first English letter for Wulfstan a few years later we find Aelfric using English for the instruction of the clergy under Wulfstan's jurisdiction.

It is fitting for us bishops to open up to you priests in English speech the learned doctrine that our canon and the book of Christ teach us, because all of you cannot understand Latin.¹⁶

The English letters from Aelfric to Wulfstan were written when Wulfstan, pleased with two Latin letters from Aelfric, requested a translation of these into English. He undoubtedly wanted them for the benefit of his clergy, but some modern readers have mistakenly seen the request as evidence that Wulfstan could not read the Latin letters.¹⁷

Aelfric's notion of translation needs some clarification. Although he himself speaks of many of his English works as renderings of Latin originals, they are rarely straightforward translations. Subscribing to the classical tradition of "sometimes word for word and sometimes sense for sense" ("nec ubique transtulimus verbum ex verbo, sed sensum ex sensu"), he often summarized, condensed, clarified, or elaborated on his sources to suit his purposes. If we compare the Latin and English prefaces to his works we find that they are not simply translations of each other and sometimes differ considerably in content -- as do the Latin and English versions of the letters to Wulfstan.

Aelfric's notion of translation differed from ours in that he felt free to add his own ideas. Nevertheless, he was concerned with accuracy of translation, particularly in matters of doctrine, and in accuracy of transmission. He often cautions scribes to copy correctly, as at the end of his English preface to the Grammar:

I ask now in the name of God, if anyone wishes to copy this book, that he correct it well according to the original; for I do not have control over whether someone brings it to error by bad copying, and it will be then his folly, not mine. Great evil does the bad scribe if he will not correct his errors.¹⁸

Aelfric also appears to have been concerned about grammatical regularity and the consistent recording of inflectional endings. Some students of Aelfric, and I include myself here, see in Aelfric's revisions and attention to fine points of grammar a conscious effort at developing or maintaining a standard form for written English.

But Aelfric's care with his writing extended beyond correctness to matters of style. During the composition of his second series of Catholic Homilies he began to develop the rhythmical alliterative style for which he is famous. Reminiscent of native Germanic verse forms, "a sentence [is] divided rhythmically into pairs of phrases, each phrase containing two accents and each pair of phrases being linked by alliteration."¹⁹ By combining expository and rhetorical techniques from the Latin

tradition and stylistic techniques from the native tradition, Aelfric wrote English prose remarkable for its clarity and readability.

Aelfric also successfully combined the two languages in response to his duty as a monastic teacher, producing for the first time a Latin grammar written in a vernacular of Western Europe. His grammatical analysis was not new; it was based on the Latin grammars of Donatus and Priscian. But his use of English to teach elementary Latin was far ahead of the times, for it was not until the seventeenth century that such grammars came into general use in England.²⁰ A study of Aelfric's grammatical terminology by Edna Rees Williams shows that Aelfric carefully explained each Latin grammatical term in English, as if teaching the Latin terminology as well as the grammatical concept. Sometimes the English explanatory word replaced the Latin, as cynn for genus, but for the most part Aelfric maintained the Latin.²¹

Although Aelfric suggests in the preface that his Grammar could be the starting point for the study of either language, his treatment of English is only occasional. For example, he asserts that the parts-of-speech system is appropriate for English: "truly in these eight parts is the entire Latin language locked up, and English fits all the parts..."²² And in the discussion of gender, he compares Latin and English.

Likewise, one ought to know that they [words] are often one gender in Latin and another gender in English. We say in Latin hic liber [masculine] and in English þeos boc [feminine]; also in Latin haec mulier [feminine] and in English þis wif [neuter], not þeos [feminine]; also in Latin hoc iudicium [neuter] and in English þes dom [masculine], not þis [neuter].²³

That the Grammar was a welcomed aid in teaching the first subject of the trivium is attested to by the fact that it survives in fifteen manuscripts. To some of these is appended Aelfric's Latin-English Glossary, a compilation of words grouped by topic ("Names of Birds," "Names of Animals," etc.) also intended to help school boys learn Latin.

Aelfric's third aid to learning Latin differs from the other two because it is monolingual. The colloquy was a common medieval device for teaching Latin as a living language to be used daily in the monastery. Aelfric's Colloquy is an elementary text to be used at the beginning of study after mastery of the alphabet, Pater Noster, Credo, and selected Psalms. The Colloquy provided young students practice in correct pronunciation and clear enunciation. It survives in four manuscripts, one of which has a continuous interlinear gloss in English added later by a teacher other than Aelfric.²⁴

Aelfric was the finest prose stylist of the Old English period as well as an influential educator. His careful attention to his native tongue set him apart from other famous teachers of medieval Europe. He was concerned with some of the same problems

that occupy the attention of the modern linguist -- problems of basic literacy, bilingual education, and definition of a standard.

Notes

¹Arnold Schröer, Die Angelsächsischen Prosabearbeitungen der Benediktinerregel (1885-88; rpt. Darmstadt: Wissenschaftliche Buchgesellschaft, 1964), Chapter XVI, p. 40.

²Ibid., Chapter XLVIII, pp. 73-75.

³Ibid., Chapter XXXVIII, pp. 62-63.

⁴Ibid., Chapter XLV, p. 71.

⁵Ibid.; and G. N. Garmonsway, Aelfric's Colloquy (1938; rpt. New York: Appleton-Century-Crofts, 1966), pp. 18-19.

⁶N. R. Ker, Catalogue of Manuscripts Containing Anglo-Saxon (Oxford, 1957), p. xiv.

⁷Helmut Gneuss, "The Origin of Standard Old English and Aethelwold's School at Winchester," Anglo-Saxon England I, ed. Peter Clemoes et al. (Cambridge, 1972), pp. 63-83.

⁸David Knowles, The Monastic Order in England: A History of Its Development from the Times of St. Dunstan to the Fourth Lateran Council 940-1216, 2nd ed. (Cambridge, 1963), p. 493.

⁹Peter Clemoes, "Late Old English Literature," Tenth-Century Studies: Essays in Commemoration of the Millennium of the Council of Winchester and Regularis Concordia, ed. David Parsons (London: Phillimore & Co., 1975), p. 109.

¹⁰Ibid., p. 110.

¹¹Ker, p. xxvi.

¹²C. R. Dodwell and Peter Clemoes, "Preface," The Old English Illustrated Hexateuch: British Museum Cotton Claudius Biv, Early English Manuscripts in Facsimile (Copenhagen: Rosenkilde and Bagger, 1974), p. 65.

¹³For the standard canon and chronology of Aelfric's works, see Peter Clemoes, "The Chronology of Aelfric's Works," The Anglo-Saxons: Studies Presented to Bruce Dickins, ed. Peter Clemoes (London: Bowes & Bowes, 1959), pp. 212-247.

¹⁴Quoted from the modern English version of the Latin preface as it appears in James Hurt, Aelfric (New York: Twayne Pub. Inc., 1972), p. 42. A modern English version of the English preface appears in Caroline L. White, Aelfric: A New Study of His Life and Writings (Yale Studies in English 2, 1898; rpt. Hamden, Conn.: The Shoe String Press, Inc., 1974), p. 101. White's book also contains a chapter which prints the Latin and Old English versions of all of Aelfric's prefaces.

¹⁵Bernherd Fehr, Die Hirtenbriefe Aelfrics (1914, rpt. Darmstadt: Wissenschaftliche Buchgesellschaft, 1966), p. 14. "Se mæssepreost sceal secgan sunnandagum and mæssedagum þæs godspelles angyt on englisc þam folce."

¹⁶Ibid., p. 68. "Us bisceopum gedafenað, þæt we þa boclican lare þe ure canon us tæcð and eac seo Cristes boc, eow preostum geopenigan on engliscum gereorde; forþon-þe ge ealle ne cunnon þæt leden understandan."

¹⁷For a fairly recent voicing of this opinion, see C. E. Hohler, "Some Service Books of the Later Saxon Church," Tenth-Century Studies, ed. David Parsons (London: Phillimore & Co., 1975), p. 72.

¹⁸Julius Zupitza, ed. Aelfrics Grammatik und Glossar (Berlin: Weidmannsche Buchhandlung, 1880), p. 3. "Ic biðde nu on godes naman, gyf hwa ðas boc awritan wylle, þæt he hi gerihte wel be ðære bysne; forðan ðe ic nah gewæld, þeah hi hwa to woge gebringe þurh lease writeras, and hit bið ðonne his pleoh, na min. micel yfel deð se unwritere, gyf he nele his woh gerihtan."

¹⁹Peter Clemoes, "Aelfric," Continuations and Beginnings: Studies in Old English Literature, ed. Eric G. Stanley (London: Thomas Nelson, 1966), p. 203.

²⁰Ian Michael, English Grammatical Categories and the Tradition to 1800 (Cambridge, 1970), p. 151, ftn. 1.

²¹Edna Rees Williams, "Aelfric's Grammatical Terminology," PMLA, 73 (Dec., 1958), pp. 456-457.

²²Zupitza, p. 11. "witodlice on ðisum eahta dælum is eal ledenspræc belocen, and ðæt englisc geðwær læcð to eallum ðam dælum..."

²³Ibid., pp. 18-19. "Ys eac to witenne, þæt hi beoð oft oðres cynnes on leden and oðres cynnes on englisc. we cweðað on ledyn hic liber and on englisc þeos boc; eft on leden haec mulier and on englisc ðis wif, na ðeos; eft on leden hoc iudicium and on englisc ðes dom, na ðis."

²⁴Garmonsway, pp. 1-17.

VII

GRAMMAR: PHONOLOGY, MORPHOLOGY, SYNTAX AND SEMANTICS

THE USE OF CONTRASTIVE STRESS IN DIALOGUE:
AN EXPERIMENT¹

Jean E. Newman

University of Toronto

In the past few years there has been a marked interest in the development of text grammars to study connected discourse. While this area is certainly not new in linguistics, the tremendous interest is. Those of us in psycholinguistics are aware of a parallel interest in psychology--there have been a number of studies on the comprehension of, and memory for, paragraphs and stories, by such researchers as Kintsch (1974), and Rumelhart (1975). But linguists are not only interested in text, by which I mean written material, they are also interested in conversation and dialogue. Moreover, especially if one considers all the work done on intonation and prosodic systems, then linguists have been interested in dialogue for many years. There is no such parallel in psycholinguistics--the study of dialogue has been thought 'too complex' due to its spoken nature. However the very acoustic variables that make conversation complex may serve to illustrate its structure.

The approach I have taken to studying dialogue is to observe the effects of the acoustic properties of an utterance on a listener's response to it. Lexical content and syntax are held constant but the stress pattern used for the utterance is varied systematically--the hope is to see a change in the contexts that listeners generate while trying to interpret the original utterance, as a function of the stress patterns that they heard. The technical considerations are not very great, as I use only a tape recorder and the ratings of naive judges to score the data. Even though I use crude techniques by the standards of such institutions as Haskins Laboratories, I have tried to develop an objective scoring technique (which I shall discuss below). With this type of a global, psycholinguistic, approach to the study of dialogue--which emphasizes the use of language by native speakers--I feel that we can learn more than by the painstaking, acoustic, approach. Perhaps at a later date, when the interesting variables have been isolated, we can develop precise paradigms, relying on technical expertise, to study dialogue more exactly. In the meantime, I think that the information that can be gained by such procedures as my own, is of interest both experimentally and theoretically. In this paper I will provide an illustrative example of one approach that could be taken to studying dialogue.

The experiment I shall discuss was motivated by Gunter's (1966) ideas about the placement of accent in dialogue. Gunter stated that a given sentence in a dialogue could be said with

accent (stress) on any of the substantive words with each stress pattern implying a particular context for that utterance, or in a neutral form (falling intonation on the last word) which implies no context in particular. Gunter's ideas seemed to be directly testable and could serve as the starting point for additional research. The experimental hypothesis was straightforward. According to Gunter's position, context sentences generated by listeners for the neutral form of an utterance should be more variable than for any of the other variations of stress placement for that sentence.

Four sentences, each four words long, and one four-word practice sentence, were constructed. These were recorded on four tapes, with one of the four versions of each of the sentences on each tape. The four test sentences were:

1. I bought some groceries.
2. She loved that dog.
3. Yesterday was very cold.
4. Mary ate Bill's cake.

An example of the four stress patterns used is given for sentence 1, the stressed word is underlined:

- A. I bought some groceries.
- B. I bought some groceries.
- C. I bought some groceries.
- N. I bought some groceries. (neutral)

(It is also possible to put stress on the last word; as the effect of this manipulation is theoretically important, I am including this stress pattern in my current research.)

Twenty-four volunteers, mostly psychology graduate students, served as subjects. Six subjects were assigned to each tape; each subject heard all of the test sentences, but only under one stress pattern. Subjects were therefore exposed to all stress patterns and all test sentences, but only to one combination of each. The test sentences were presented singly and subjects were asked to generate, on my instructions, either a sentence which could precede it (before) or follow it (after) in a conversation. Before hearing the test sentences, subjects generated both a before and an after context for the practice sentence. Each subject generated two before and two after contexts. They were told to try to produce complete sentences and that they could take either the same role as the speaker in the test sentence, or the other role.

The before/after variable was included because Gunter (1966) had stated that "A given sentence, then, points backward to its context....the converse is not true. Given a dialog up to a certain point, we cannot predict anything at all [his italics] about the next speech, for the next speech may not be relevant to what is said" (p. 33-4). My subjects interpreted the instructions concerning after contexts 'following in a conversation' to mean that

there was a next utterance and that it was in fact relevant. While it is obviously true that speakers sometimes change topics in a conversation, it is not nearly as common, nor as abrupt, an occurrence as Gunter states.

Below are examples of contexts generated for each of the stress patterns of sentence 1 (generated independently by eight subjects):

- A. (before) I was terribly hungry.
 (after) Even though I had to borrow some money to do it.
- B. (before) What were you doing downtown today?
 (after) And they were very expensive.
- C. (before) What did you bring back from the market?
 (after) But I forgot to get potatoes.
- N. (before) I went to the store.
 (after) What did you do with the groceries?

It is not obvious at first glance how to score these sentences for their contextual information. Introspection, at least in psychology, is not a commonly accepted measure of data. A scoring method was devised to circumvent this problem. The procedure rests on the assumption that each sentence could have come from a larger context which I term the "script" of the situation (this term is borrowed from Schank (1975) but our usages differ somewhat). One might imagine that for sentence 1, prior to buying groceries 'I' became hungry and that it was this context that prompted the action described in the test sentence. Becoming hungry is thus the script used by the speaker in example A-before above; it sets the stage for the next utterance in the dialogue which was the test sentence I bought some groceries (with stress on the first word). In contrast, even though 'I' may be telling someone about buying groceries, 'I' may be more interested in telling them what was bought. The test sentence, in this script, could be uttered before getting onto the business at hand, which is enumerating the list of groceries in an after sentence. All the scripts used to score the generated context sentences of sentence 1 are given below, similar sets of scripts were composed for the remaining three test sentences:

- (1) becoming hungry
- (2) collecting money (financial considerations)
- (3) coming or going to a store
- (4) buying while at the store
- (5) what was bought
- (6) making a meal
- (7) general remarks about shopping
- (8) other (sentences that can't be classified).

My interpretation of Gunter's position would predict that the scripts that could be assigned to context sentences should be consistent for any stress pattern except the neutral. The scoring procedure was thus as follows: first a set of scripts was generated for each test sentence, then each context sentence was assigned to a particular script (according to the reasoning above, example A-before is an instance of script (1), and A-after is an instance of (5)).

The statistic used to measure the variability of context sentences produced for each stress pattern comes from information theory. The idea of using it in this type of situation (i.e. to measure variance across nominal categories) was suggested by a similar application by Kolers, Park, and Dell (in press). The statistic determines how much of the available uncertainty (variability) is used up by the categorisation of context sentences into scripts and is sensitive to the total number of scripts actually used (and not to the number listed). It is written as

$$\frac{\text{Total Uncertainty}}{\text{Maximum Uncertainty}} = \frac{\sum P_i \log P_i}{\log (1/k)}$$

where k is the number of context sentences generated for that stress pattern (always 6) and P_i is the obtained proportion of responses for a particular script (i). Maximum variability (six contexts assigned to six different scripts) would be equal to 1; minimum variability (six contexts assigned to the same script) would equal 0, with a range of values between them, depending on the pattern of assignments of contexts to scripts. The statistic was used to compute separately the variability of context sentences for each stress pattern of each test sentence. Using my own assignments of context sentences to scripts (or ratings of the context sentences), the mean variability for each stress pattern was computed. Averaged over all four sentences, the means were: A = .79, B = .60, C = .51, N (neutral) = .77. Recall, the higher the value, the more variable were the context sentences. An analysis of variance performed on the means of the stress patterns for each test sentence was significant, F (3,9) = 13.70, p < .01.

There is a serious possibility of bias in the results reported above, as I (as rater) was not unaware of the original stress pattern of the test sentences while scoring context sentences. To remedy this possible confound, I asked five naive subjects (all psychology graduate students) to rate the context sentences as I had done but unlike me, they were blind as to the original stress pattern of the test sentences. Their mean variability scores (averaged over all five raters) gave the same pattern as my own: A = .80, B = .68, C = .63, N = .73. The data were combined with my own and an analysis of variance was performed. The analysis revealed significant differences: using a subject based error term, F (3,15) = 10.82, p < .01; using a sentence based error term,

$F(3,9) = 5.43$, $p < .05$. Using a more conservative statistic, F' , the differences were still shown to be significant, $F'(3,17) = 3.92$, $p < .05$. The F' statistic allows one to generalise the results over sentences of the type used in this experiment, but it must be done with caution as the sample used was very small. The data are plotted for each sentence (indicated next to the corresponding line on the graph) as a function of each of the stress patterns used, in Figure 1.

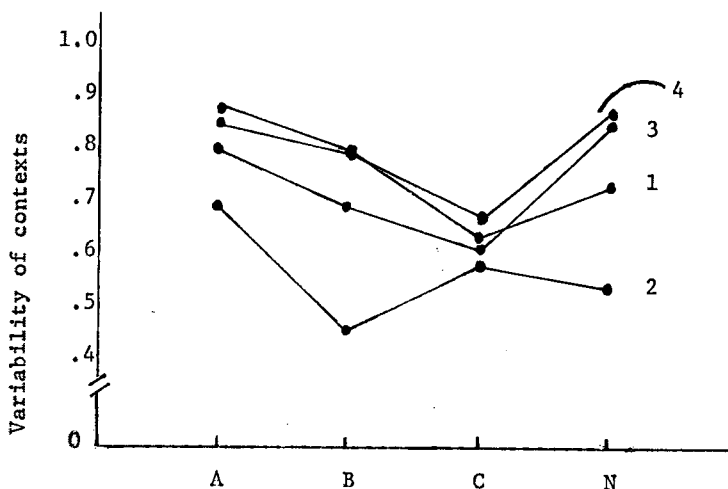


Figure 1. Variability of context sentences as a function of contrastive stress in the test sentence (higher values indicate greater variability).

Gunter's (1966) idea that the role of stress in dialogue is to serve as a contextual signal, has been substantially supported. Neutral sentences did result in more variable contexts than the same sentences with stress on either the second or third word. However, stress on the first word also resulted in variable contexts. The reason that this happened, I think, is the same reason that Andrew's (1977) subjects, in an experiment on the relative importance of word order and contrastive stress as focusing devices, rated the first word as most important. According to Andrew's results, the first word of a sentence is already semantically prominent and the addition of stress does not add as much of an increase in prominence as it does to the other words in the sentence (she also stressed the last word as well as having a neutral form). The explanation I offer is that the first word of a sentence is, in Halliday's (1967) terms, the theme of the sentence. According to Bates (1976), the theme selects "a point of reference in discourse" (p. 169). It may set the stage for an utterance, but it does not provide specific contextual signals

which would imply a particular context for the utterance. The addition of stress on subsequent words narrows the realm of discourse quite specifically, the boundaries of which have been pointed out by the theme in a general manner. Stressing the theme is in some way redundant--it points out that the word is to be noticed, but it is hard for the listener to know what to do with it. The stressed theme could be used contextually in either an echoing or an argument situation, but these types of contexts are relatively rare. Subjects probably did not pay much attention to the stress in my experiment, rather generated sentences which could fit within the general theme but which had no one overriding context. Therefore, I think that the role of the theme in dialogue is an important one--it serves to narrow down the topic of conversation--but it does not provide specific contextual information to link sentences from one to the next. Clearly these suggestions concerning the theme are just that, and need to be investigated experimentally.

An interesting prediction regarding the variability of contexts generated for neutral sentences can be formulated from Halliday's (1968) theory. According to Halliday, the last constituent of a sentence is generally "new" information (this is not the only constituent of a sentence that can be new, however) both when marked and unmarked. In my experiment the neutral form provides a case of an unmarked last constituent, which should therefore be new. It seems reasonable to expect that context sentences should be constrained in their content so as to focus on the new information in the next sentence, in the case of before contexts, and to expand on this new information in the case of after contexts. No such thing happened in either case. Andrew (1977) found a similar result--the last word, both unmarked, and marked by stress--was judged as the least important word in the sentence. The finding that the neutral form of a sentence leads to variable contexts, combined with informants' judgments that the last word of a sentence is least important, seems to put the information value of the last constituent in a dubious light.

It was mentioned earlier that subjects, contrary to Gunter's (1966) expectations, were able to formulate an after response that was relevant to the test sentence, with no difficulty. Gunter seemed to feel that because the participants in a conversation cannot know for certain what is to come next, then "we cannot predict anything at all (his italics) about the next speech" (p. 33-4). The data from this study suggest that subjects have none of Gunter's temerity. The patterns of responses for the types of contexts, before and after were the same:

(before) A = .87 B = .79 C = .78 N = .85

(after) A = .95 B = .56 C = .61 N = .72

(nothing can be made of the absolute differences as the samples are too small--another question for further research). This

experiment, in direct contradiction to Gunter's assertions, demonstrated the ability of a stress pattern to limit the content of the next utterance as well as its ability to point backward to its preceding context. Thus, regardless of their ability to correctly predict the next utterance, the participants in a conversation do generate expectations concerning the content (and possibly the stress pattern) of the speaker's next sentence.

The reader might suppose from the results of this experiment and the discussion above, that I am proposing stress as the major, if not the only, contextual link in dialogue. Such a proposition would be false, and one has only to return to Figure 1 for an example where stress loses out to the lexical and syntactic components of the sentence. The pattern of results for sentence 2 is obviously quite different than for the other three sentences, which in fact resemble each other quite closely. I think that this discrepancy is due to the use of the past tense of the verb (love), She loved that dog. When the verb was stressed, 24 out of the 36 possible ratings of the contexts for that stress pattern were judged to be about the loss of the dog, usually by death--subjects produced such sentences as It got fleas and died and some where the dog wasn't actually dead, The dog ran away from home. Even when no outright mention of the loss of the dog was made, it was implied, Yeah, that was a shame. This consistent focusing on the demise of the dog obviously lowered the variability of responses. These types of contexts were even generated in the neutral case; 21 judgments were about the dog's demise, e.g. Its death caused her terrible grief--hardly a neutral sentence which implies no particular context! Again, variability was abnormally low. This pattern persists with contexts generated for the first stress position, though not as strongly; 15 judgments were about loss, e.g. It's a shame that it got run over by a car last night. It might be that there is some effect of stress operating when the first word is focused, since there was greater variability than in either of the other cases. Perhaps the stress distracts subjects from the implications inherent in the use of the past tense of love. The constant use of contexts about the loss of the dog completely disappears when the third word, that, is stressed. None of the responses were judged to be about the dog's demise. In light of these findings, it would certainly be worthwhile to investigate the effects of stressing the last word, dog, as well. In general, a future research strategy should be to investigate the differential, and interactional, roles of the propositional content (lexical and syntactic) and the stress pattern of a utterance in determining a context grammar for dialogue.

Finally, I have some doubts concerning the use of the term contrastive stress to describe the use of differential intensities on words in an utterance. Gunter (1966) noted that this term has caused a lot of confusion in the literature, and prefers to talk of shifting accent in dialogue. Using contrastive stress implies that the word being said with greater intensity contrasts with

another word (either implied or stated earlier) which could have been inserted in its place. Placing a word under contrastive stress, according to Chafe (1976), tells the listener that it is a correct choice and that the alternative word would have been false. Chafe suggests using a "not" test to determine whether a word has been contrastively stressed, e.g. JOHN (not Henry) stole the book. Contrastive stress seems to be one of several forms of stressing words in an utterance but it has very particular connotations. In my own experiment, I could be described as using contrastive stress on the nouns in the test sentences, but one could hardly say the same for all of the verbs. What is the contrast for ate in Mary ATE Bill's cake? Surely not Mary DRANK Bill's cake! At some level of description it is true that stressing ate does imply that Mary did not drink the cake, but it is hardly a normal use of the term contrastive. I would prefer to think that stressing the verb conveys something of the emotional state of the speaker and his/her certainty that the event took place. A similar situation holds for some and very (that and Bill's are clearly contrastive by the "not" test). Both some and very contrast in some sense ("not" none, "not" slightly) but their main function is to tune the noun being modified along some dimension (I borrowed this term from Bates (1976) who talks, in a related way, about analogue "tuning" of propositions in stressed sentences). One could imagine that the utterance Yesterday was very cold describes a day that was 25° (F), whereas Yesterday was VERY cold was said about a day that reached -5° (F). Because of the different semantic entailments of the various types of stress, I prefer not to use a single term such as accent to describe stress patterns (also, Gunter seems to use accent to apply to single syllables, it is not clear what term would apply to multi-syllabic words). Perhaps the title of this paper is a misnomer; rather than studying the use of contrastive stress in dialogue I am concentrating effort on the variety of contextual devices such as contrastiveness, certainty, and tuning, that it is possible to employ by imposing different stress patterns on an utterance.

In conclusion, I feel that Gunter (1966) was correct when he said that the placement of accent, or stress, in dialogue can point to the contextual origin of an utterance. Further, I feel that it can direct the flow of subsequent conversation. Accent placement, I think, reaches both forward and backward as a contextual link in dialogue, one of many links which deserve further research, both of a theoretical and an experimental nature. The search for a context grammar for dialogue lends itself easily to psycholinguistic experimentation, and in doing so, I hope leads us to new linguistic theories.

References

- Andrew, C. M. An experiment on grammatical focus. Paper presented at the Fourth LACUS Forum, Montreal, 1977. To appear in this volume.
- * Chafe, W. L. Givenness, contrastiveness, definiteness, subjects, topics, and point of view. in Li, C.N. (ed.), Subject and Topic, New York: Academic Press, 1976, 25 - 56.
- Gunter, R. On the placement of accent in dialog: A feature of context grammar, Journal of Linguistics, 2, 1966, 159 - 179. Reprinted in Gunter, R. Sentences in Dialog, Columbia, S.C.: Hornbeam Press, 1974.
- Halliday, M. A. K. Notes on transitivity and theme in English. Part 2. Journal of Linguistics, 1967, 3, 199 - 244.
- Halliday, M. A. K. Notes on transitivity and theme in English. Part 3. Journal of Linguistics, 1968, 4, 179-215.
- Kintsch, W. The Representation of Meaning in Memory, Hillsdale, N.J.: Lawrence Erlbaum Associates Inc., 1974.
- Kolers, P., Park, N. & Dell, G. On sentential anagrams: Making sentences out of words, Journal of Psycholinguistic Research, in press.
- Rumelhart, D. E. Notes on a schema for stories. In D. G. Bobrow and A. Collins (Eds.), Representation and Understanding, New York: Academic Press, 1975.
- Schank, R. C. The role of memory in language processing, Paper presented at the 1975 meeting of the American Association for the Advancement of Science.
- * Bates, E. Language and Context, New York: Academic Press, 1976.

Footnotes

¹The author was supported by a postgraduate scholarship from the National Research Council of Canada during the period of this research. I would like to thank Christine Andrew, Gail McKoon, Sandra Trehub, and particularly Gary Dell, for their helpful discussions.

PHONOLOGICAL ASPECTS OF TIMING

Sandra C. Browne¹

The University of Michigan

This paper is a report on a preliminary study of the contrastive timing of three languages: Indonesian, French and American English.

Working with some Indonesian conversation, I began to suspect the claim that Indonesian is a syllable-timed language. Its timing, when compared to French, another language claimed as syllable-timed, is perceptually quite contrastive. I decided to investigate the timing of these two languages, plus American English. My working hypothesis is shown in Figure 1.

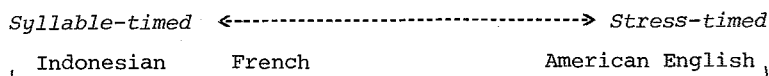


FIGURE 1.

Initially I expected that French would show similarities to Indonesian in the general pattern of its timing, but that differences would emerge which would establish the need for a continuum for the timing of these languages as opposed to the categories 'syllable-timed' or 'stress-timed'.

To discover the timing mechanisms of each language, two methods of analysis were chosen: the first, instrumental; the second, perceptual. As is shown in Figure 2 the results of the instrumental analysis were surprising. Using the PAUSE-GROUP as the delimiting locus for an analysis of timing, the dichotomy 'syllable-timed' vs. 'stress-timed' collapsed. The instrumental analysis places American English BETWEEN French and Indonesian in terms of regularity of timing. Indonesian shows the least variation WITHIN pause-groups, American English next and French the most variation of timing within pause-groups. The variability in overall syllable length as measured within a pause-group shows Indonesian to have the most consistent syllable length, American English next and French the least uniform syllable length of the three. (The hatch marks in Figure 2 show the adjustment in syllable length BETWEEN pause-groups; that is, the length of syllables is adjusted internally for the pause-group with the greatest timing adjustment for English (42%), the next for Indonesian (36%) and the least adjustment in French (11%) when the final syllable of each pause group is omitted.)

THE DATA. The data from which these results were obtained consists of sample dialogues which were tape-recorded by native speakers of the three languages. The original conversations were adapted from John Wolff's text for beginning Indonesian and were recorded by two native speakers of the Javanese dialect. The dia-

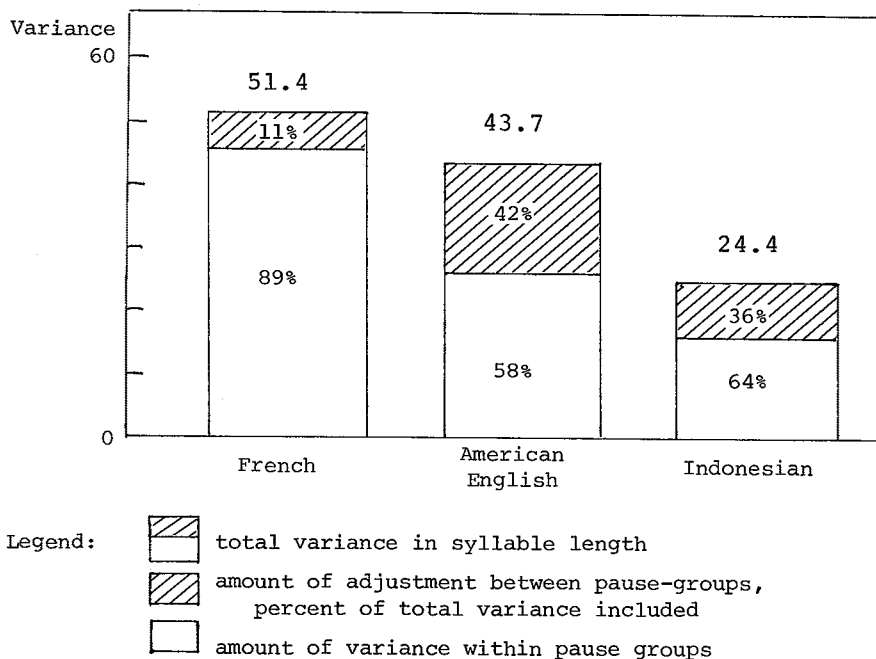


FIGURE 2. Sources of variance in syllable length for French, American English and Indonesian; final pause-group syllables omitted.

logues were then translated into French and American English and recorded by native speakers of those languages. (The artificiality of translating from one language to another is recognized, but was allowed in order to preserve homogeneity of context in the data. For stylistic reasons liberties of translation were permitted, most strikingly in the change of cuisine from Djokjakarta to Djakarta in the French and American English dialogues.)

The dialogues concern everyday affairs common to all three languages and cultures: going on a picnic, finding a place to live, eating in a restaurant. The portion analysed for this paper is a paragraph-medial portion of one of the dialogues, which was recorded toward the end of the taping procedure when the native speakers were more relaxed and had presumably mastered the task of reading dialogue and assuming the roles of the participants. Two tapes were made. In all cases the second taping was used for analysis, to insure familiarity with the situation (for the speakers). The reader should bear in mind that the sample is small (six native speakers—two for each language—of which only three speakers are represented here). Also, because the dialogues were read, they do not constitute entirely spontaneous speech. This fact is balanced to some degree by the nature of the material.

Sample sentences from the three languages are shown on the following page:

- 1) *Ja, restorannja sih sederhana, tetapi makanannya makanan Djokdja.*

2- 3 2- 1 / 2- 3 2- 1 //

'As for the restaurant, you know, it's simple, but the food is Djokdjakartan'

- 2) *A vrai dire, le restaurant est simple, mais la cuisine est bonne*

2- 3 2- 3 2- 1 2- 3

et elle vient de Djakarta.

2- 3 2 1//

- 3) *The restaurant's plain, but the food is Djakartan food.*

2- 3 2 1 / 2- 3 2- 3 1 //

THE INSTRUMENTAL ANALYSIS. The apparatus used for the instrumental measures was an ink-jet oscillograph set up to display the amplitude curve for the speech wave and run at two speeds: 50mm/sec for the purpose of pause-group analysis and 200mm/sec for syllable length analysis. A portion of the sample was re-analyzed using a computer which produces a digitized printout of the speech amplitude averaged over 1 centisecond (csec) intervals. The results of the instrumental analysis are presented in Tables 1 and 2.

Language	#PGs*	##s*	Total csec	Mean \$ length	s.d.	##/sec
Indonesian	11	103	1290	12.52	4.95	7.98
French	7	100	1633	16.33	7.04	6.12
Am. Engl.	10	81	1318	16.27	6.98	6.14

TABLE 1-A. Syllable length including final syllable of a pause-group.

Language	#PGs	##s	Total csec	Mean \$ length	s.d.	##/sec
Indonesian	11	92	1142	14.65	4.94	6.82
French	7	93	1515	16.29	7.07	6.14
Am. Engl.	10	71	1119	15.76	6.61	6.34

TABLE 1-B. Syllable length excluding final syllable of pause-group.

Language	#PGs	# Syllables	Total csec	s.d.
Indonesian	11	103	1290	2.37
French	7	100	1633	1.90
Am. Engl.	10	81	1318	4.62

TABLE 2-A. Effects of pause-group on syllable length including final syllable.

* PG = Pause-group; \$ = Syllable

Language	#PGs	# Syllables	Total csec	s.d.
Indonesian	11	92	1142	2.97
French	7	93	1515	2.40
Am. Engl.	10	71	1119	4.25

TABLE 2-B. Effects of pause-group on syllable length
excluding final syllable.

The analysis of syllable length was not morphophonemic. The measure taken was from the initial vowel peak of one syllable to the initial vowel peak of the next, continuing through the string until the signal ended, indicating a pause. Using the definition of the syllable proposed by O'Connor and Trim² we measured from syllable nucleus to syllable nucleus. This measure was taken in millimeters and then converted to centiseconds. The measure for pause-groups was taken from silence to silence and therefore constitutes the speech signal between two silences. No syntactic considerations were allowed in this analysis. If two sentences were run together by the speaker, they were then counted as a single pause-group. (This is most evident in the French sample, where the total sample time is longer than for Indonesian or English but the total number of pause-groups is fewer.) Hesitations were treated as pauses in this analysis, but there were few of them, and only one in the data treated here.

To arrive at the timing of the three languages, four measures were taken. Table 1 shows the syllable length. Reading across from left to right, the language is given, then the number of pause-groups, the number of syllables in the sample, followed by the total length of the sample in csec. From these figures, the mean syllable length for each language, the standard deviation of syllable length for the total sample and the number of syllables per second were calculated and are shown in the last three columns of Table 1.

Looking at Tables 1-A and 1-B we see that the standard deviation in syllable length for French exceeds that of the other two languages, even when the final syllable of a pause-group is excluded. Moreover, when the final syllables of pause-groups are excluded the MEAN syllable length remains nearly the same. This indicates that the final syllable in French is NOT important to pause-group timing.

Table 2 shows the pause-group effect on syllable length for the three languages. This measure was arrived at by averaging the number of syllables within a pause-group and computing the standard deviation. When the final syllable is included in the measure, as is shown in Table 2-A, the deviation decreases for all three languages, with French showing the greatest reduction in deviation (a reciprocal view of Table 1).

Averaging syllables within pause-groups gives more weight to small pause groups, in that the average within a pause-group containing a small number of syllables is proportionally greater than for a pause-group with a large number of syllables. For example, in the Indonesian data the first sentence consists of three syl-

lables *Ja*, *ada* 'Yes, there is.' In the instrumental display, however, the final /a/ of *Ja* and the initial /a/ of *ada* fused, creating a long vowel [a:]. This fusion results in a pause-group consisting of two syllables with a total pause-group length of 39 csec. Other pause-groups in the data for Indonesian contain as many as 18 syllables and last as long as 219 csec.

The relation between the amount of deviation in syllable length ACROSS pause-groups as shown in Table 2 and the amount of variance WITHIN pause-groups in Figure 2 is particularly salient for American English. As a so-called 'stress-timed' language, English is supposed to have long and short syllables which may be stressed or unstressed. The claim can be measured instrumentally for individual segments. However, speakers seem to adjust the timing of syllables so that pause-groups are of relatively equal length. When the pause-group is used as the unit, adjustment for syllable length ACROSS pause-groups is so great that 42% of the syllables in American English have been adjusted to provide a rhythmic regularity which is greater than that of Indonesian and much greater than that of French. It may be this adjustment of syllable length which makes the rhythm of English SOUND regular.

THE PERCEPTUAL ANALYSIS. Timing is only one part of an analysis. Perceptually, timing may be affected by various factors: intonation contours, long syllables or other boundary markers. To look at these other factors, I employed methods based on K.L. Pike's analysis of the intonation of American English (Pike 1945). The sample sentences (given earlier in this paper) are annotated for pitch change, pitch level and pause, with the number 3 indicating higher pitch and the number 1 low pitch. Because pitch change is relational, the pitch level for each syllable is not indicated. Rather, a given pitch level continues until a perceived pitch change occurs. For example, in the Indonesian sentence *Ja, restoran-* has the same relative pitch when contrasted with *-nja* which is perceptually higher. Pause groups are indicated by / for non-sentence final and // for sentence final.

For American English, the stressed syllable is indicated with the marking /' IF SUCH STRESS WAS PERCEIVED.

In the sample sentences, two added cues to the rhythmic structures of the languages present themselves: pitch change and vowel length. Examining pitch change, we find that for Indonesian there is pitch rise on the TOPIC of the first pause-group (the final syllable of *restoranjja*), on the final syllable of the clause introducer *tetapi* 'but' and on the final syllable of the TOPIC of the second pause-group. There are, then, two pause-groups, the first ending with the word *sederhana* 'simple', the second ending the utterance. Syntactically, however, there are three pause-groups in the text—the third being the initial syllable *Ja*. While instrumentally there is no pause between *Ja* and *restoranjja*, there IS lengthening of the vowel of *Ja*. In the overall timing of the utterance the first syllable is considerably longer than the other syllables in the pause-group.

The contrast of the instrumental analysis with the perceptual

one produces ETIC and EMIC conflict in the determination of pause-groups. Syntactic boundary markers do not coincide with acoustic ones and intonation is not the rhythmic cue we expected. (I use Pike's terms *etic* and *emic* here because I find them entirely appropriate.)

The French data presents much the same results. The first syntactic phrase of the sample sentence *à vrai dire* is not followed by a pause, nor is there a pause after *simple* or *bonne* as would be posited by a sense-group analysis such as Armstrong's (1974).³ The French sentence presented here has one pause-group, with several internal 'sense-groups', each corresponding to a pitch change. These 'sense-groups' are, by our definition, not syntactic, but intonational in nature. Note that the 'sense-group' *est simple* has a fall after the rise in pitch which indicates a syntactic boundary marker. Here as in Indonesian, the clue to the timing of the language is not just pauses, but the combination of pitch rise plus syllable length. (Instrumentally *simple* has the vowel structure of two syllables, that is /sẽmplə/ instead of /sẽmpl/ for this data.)

For American English, pitch change and stress correlate for pitches which rise, but not for those which fall. Perceptually the final syllable *plain* in *the restaurant's plain* and the second instance of *food* are unstressed. These syllables are longer, however than any of the stressed syllables in the utterance. So perceptually, syllable length, intonation and stress are all linked as cues to timing for American English. In some cases, only one of these variables gives the clue, as is the case in the second pause-group, where only syllable length gives a clue to timing. (Syntactically, the second occurrence of *food* is redundant and could be deleted from the string.)

CONCLUSIONS. Having looked at and compared the instrumental and perceptual data, what conclusions can we draw for theoretical and practical purposes? Theoretically, we have argued that there is a conflict of ETIC and EMIC in spoken language. In the vernacular, what we hear is not identical to what is there, and certainly for the native speaker, not what he has been TAUGHT TO BELIEVE is there. For the foreign speaker, the timing of the speech signal is varied, stresses are suppressed which 'should be there', and intonation contours do not always coincide with rhythmic expectations.

For American English, the conflict between a rhythm-group analysis such as Pike's and an essentially isochronic analysis like M.A.K. Halliday's is evident in the data. Both are correct, for different reasons. The adjustment for regularity of pause-groups indicates that the timing of American English is, in fact, more or less regular if viewed ACROSS pause-groups. Internally, however, syllable length is irregular...more so than for Indonesian, but less than for French! It is the lack of adjustment within pause-groups which leads us to assume that French is 'syllable-timed' even though syllable length is more variable.

Investigation of the rhythmic timing of languages at the pause-group level, given the heuristic employed in this analysis, suggests a phonotactic model based on connected speech rather than a model based on citation forms. Instead of trying to predict where stres-

ses fall, deriving the prediction from underlying forms, we look at where they DO fall, perceptually and instrumentally, and thus come at the problem of rhythm from a higher level of the phonological hierarchy than is usually employed.

In language, as in music, the text (that is, the score) and the spoken form (the piece) are not synonymous. If one is to derive the one from the other, an analysis of text alone is not enough. As is shown in this paper, the difference in the rhythmic patterns of these three languages points to the significance for the foreign language learner of some knowledge of comparative rhythmic patterns. The development of teaching materials which include comparative phonotactics for both the native and the target language is in order. And the distinction 'syllable-timed' vs. 'stress-timed' is not sufficient to the task.

NOTES

¹ My thanks to Laurence J. Krieg of The University of Michigan for helping me with the instrumental analysis and for performing the computer analysis, as well as critiquing the present version. To Kenneth Pike and M.A.K. Halliday I owe my interest in the problem presented here. Individual help comes in many forms (statistical, critical, stylistic and supportive) and I must thank others who have given time and effort: Prof. James W. Downer, Prof. Kenneth Hill, Prof. John Upshur, Prof. Richmond Browne, and Nancy Conklin.

² The definition of the syllable used here is that of O'Connor and Trim as presented in Fischer-Jørgensen (1975:375). They define the syllable as 'a minimal pattern of phoneme combination with a vowel unit as nucleus preceded and followed by a consonant unit or permitted consonant combination'.

³ Armstrong defines a SENSE-GROUP as 'a group of words (sometimes of one word only) having in itself a certain sense, not necessarily complete' (1974:133). This definition, which is of necessity vague, is basically semantic but functionally syntactic in that her 'sense-groups' correlate with constituent boundaries like noun phrase, verb phrase and prepositional phrase.

REFERENCES

- ARMSTRONG, Liliás E. 1974. The phonetics of French. London: G. Bell and Sons, Ltd.
- FISCHER-JØRGENSEN, Eli. 1975. Trends in phonological theory. Copenhagen: Akademisk Forlag.
- HALLIDAY, M.A.K. 1970. A course in spoken English: Intonation. London: Oxford University Press.
- PIKE, Kenneth L. 1945. The intonation of American English. Ann Arbor: The University of Michigan Press.
- WOLFF, John U. 1971. Beginning Indonesian. Ithaca: Cornell University.

THE "ENCLITIC"¹ SYLLABLE IN ENGLISH

William J. Sullivan
University of Florida

The phonology of English has received serious attention in major studies from linguists of disparate schools during the past 30 years, e.g. Trager and Smith 1951, Cohen 1965 (1952), Chomsky and Halle 1968, and Cygan 1971. All these works intend a complete description of English phonology, except for Cygan 1971, which has a more limited aim. Nonetheless, certain problems of English phonology remain disputed, and certain other problems have received arbitrary or highly complex and unconvincing descriptions. If we consider the details of specific dialects, the problems increase.

For example, no straight-forward description of the distribution of 3- vs. 4-stress has ever been given (cf. the discussion in Gleason 1955:323). The distribution of vocalic liquids and nasals, which should somehow be predicted, has been subjected to arbitrary description (cf. Bloch & Trager 1942). Another question concerns schwa. Schwa satisfies the biuniqueness requirement and is therefore phonemic in this sense. But its restriction to 4-stress syllables and its occurrence as an allophone of many (all?) vowel phonemes suggest that its appearance should be predictable.

The above problems occur in all dialects of English, so far as I am aware. Consider as well data drawn from my own central Connecticut dialect (my wife and other speakers were available as informants). The occurrence of unaspirated unvoiced stops, 4-stress tense vowels, and the apical flap are among the most prominent of such data. Such data from all dialects are necessary for a complete description of English phonology. The Connecticut dialect data are included here simply to flesh out the description for a particular dialect and to indicate where variations occur. No claim for the exhaustion of dialect material is intended for the present study.

The underlying assumption here is that all these phonological problems are interdependent and can be explained in terms of a single integrated minor syllable type called the "enclitic" syllable.

The description of the syllable is couched in terms of segments for the sake of convenience. Though fully stratificational, it is without diagrams, as it has not been completely formalized in relational-network notation. The diagrams would demonstrate that the description is formally non-segmental and, as with my other work in phonology, is fully compatible with non-segmental phonologies like those of Sampson 1970, Griffen 1975, and Griffen 1978 (in the present volume).

But I return to theoretical considerations below. I must first describe the phonotactic structure of this minor syllable type, its phonotactic distribution with respect to the major syllable type,² and the archiphonemic neutralizations (or phonemic contrasts suspended) in this environment. Finally, I consider some implications for phonological description and theory.

I. The phonotactic structure of the "enclitic" syllable

The independent or major English syllable has an obligatory peak with optional onset and coda. There are numerous additional stipulations which need not concern us (cf. Cygan 1971). The "enclitic" syllable, however, has two obligatory positions: initial and final.

In initial position, the "enclitic" syllable can have any single consonant phoneme, though the allophonic variation is considerable. Unvoiced stops are never aspirated, though the fortis-lenis opposition is generally maintained (cf. Section III below for archiphonemic neutralizations). The stops are unreleased and possibly glottalized before homorganic nasals. In some dialects, /t/ is realized as a glottal stop. The only common nasal in this position is vocalic [ŋ], as in *hearten* and *harden*, but *happen* may be pronounced with [m] and *dragon* with [ŋ]. If so, the second syllable of each is "enclitic".

For the central Connecticut dialect, the most salient characteristic of the "enclitic" syllable initial is the neutralization of /t/ and /d/ everywhere except in contact with nasals or after /l/. They are realized as [D] with lateral release before [l] and as a voiced apical flap [ɾ] elsewhere. I return to this point in section III below. But this neutralization is important here as a signal of the occurrence of the "enclitic" syllable, and it permits a determination of the phonotactic structure of the final.

For most dialects of American English, the vocalic liquids and nasals may act alone as the final, e.g. *battle*, *batter*, *harden*, *hearten*, *happen*, and *dragon*. Schwa is also widespread: *sofa* [sow fə]. The central Connecticut dialect also has [i] and [o]: [sɪ ɰi] and [fow ɰo] for *city* and *photo*. The second vowel in each case is tense but short, undiphthongized, and 4-stress. Each is preceded by the apical flap. The normal realizations of these vowels are [i:] and [ow], respectively.

Many of these data are in materials on English phonology, but not in an integrated description. The information about reduced or transition vowels is found in one area, and the information on the consonants in another (cf. Cohen 1965:75-78 and 65-71). However, the present description still needs to be expanded in two ways. First, the final as outlined above may optionally be extended by /z/ or /d/. These are the realizations of the desinential morphemes relevant here: /z/ realizes the plural, possessive, and third person singular, present tense morphemes; /d/ realizes the unmarked past tense morpheme. It is possible that other morphemes should be realizable here. If so, the position exists, and they can easily be accommodated.

Second, another final type needs to be accounted for. This type has an /I/ followed obligatorily by a single non-semivowel

consonant, by a single non-semivowel consonant plus (plural) *s* or (past) *t*, or by an /st/ cluster, e.g. *phonetic(s)* [fow nE ØIk(s)] or *fetish* [fE ØIʃ]. This includes the realizations of the past tense and superlative morphemes after apical stops, e.g. *waited/waded* [wey ØId] and *hottest* [ha ØIst]. The morphological and morphophonemic questions involved here (inserted vs. deleted vowel, morphological marking, etc.), though interesting, are beyond the scope of this study.³ Suffice it to say that the phenomenon is not limited to inflectional endings: *fetid* [fE ØId].

The vowel I produce in *fetid* is clearly a front vowel. I have heard speakers from other dialects whose vowel has just as clearly the central [ɪ]. The place of articulation causes no problem. It is fixed for each dialect by its position within this particular final of the "enclitic" syllable.

There are many advantages in expounding a minor syllable type of this sort. But before they are outlined, it is necessary to define the phonological environments in which the "enclitic" syllable may occur and to outline the archiphonemic neutralizations.

II. The phonotactic distribution of the "enclitic" syllable

In general, the "enclitic" syllable can optionally follow any independent or major syllable. There are restrictions on what may be in coda position of the major syllable. Coda position may be empty, as in *little* [lɪ Øl]. It may be filled by a semivowel, as in *ladle* [ley Øl] or *coda* [kow Øə]. It may contain a nasal which is always homorganic to a following stop, as in *gamble* [gæm bl], *simple* [sɪm pl], *handle* [hæ̃n dl], *cattle* [kæ̃n tl], *single* [sɪ̃n gl], and *twinkle* [twɪ̃n kl]. These words have vocalic [l] and unaspirated unvoiced stops, but there is no neutralization of the morphons *t* and *d*. The coda position may be filled by a liquid, as in *shelter* [ʃẼl tr] or *portion* [pɔ̃r ʃn]. But note that the /t/ in *shelter*, though unaspirated, is not neutralized with /d/. In many dialects, including that of central Connecticut, *carter* and *carder* are homophones with the apical flap: [kar Ør]. Nothing other than these six choices is available; an obstruent cannot occur.⁴ Compare *fatter* [fæ̃ Dr] with *faster* [fæ̃s tʰæ̃] and *adopted* [ə̃ dap tʰId]. The latter two have aspirated /t/, showing no "enclitic" syllable (and further demonstrating the inadequacy of a strictly segmental approach, which would predict an unaspirated /t/ after /s/). The last syllable of each of these three words is unaccented. But *fatter* has a fully-reduced vocalic [r]. The other two have short but unreduced vowels. This is a formal description of the 3- vs. 4-stress contrast which predicts the contrast directly: an unaccented major or independent syllable has 3-stress; an "enclitic" syllable has 4-stress.

Again, 4-stress on the "enclitic" syllable implies no accent and no tone. This constitutes a second restriction on the occurrence of the "enclitic" syllable. A major syllable can have primary accent (1-stress), secondary accent (2-stress), or none at all (3-stress). A regular alternation based on morphological assignment of accent can thus be observed. The adjective *able* is realized via the morphonic shape *a:bɪl*. There is no morphologically assigned accent, so the phonology realizes it as [éy bɪl],

with phonotactically determined accent on the first syllable. The nominalizing suffix *ity* includes a specification of accent on the syllable preceding it, so *ability* is realized as [ə bɪ lə tʰi:] or [ə bɪ lɪ ði]. These alternate possible encodings point to another interesting phenomenon, variation in syllabification during encoding, which is also beyond the scope of this study. But the main point is that the assignment of accent to the second syllable prevents its being realized as "enclitic".

There are also restrictions on the "enclitic" syllable which are due to morpheme composition. The adjectival *gambling* contrasts with the progressive participle of the verb (cf. Cygan 1971:106-8 for full discussion). Morphological justification is necessary, of course, but I would ascribe the difference to the occurrence or lack of a desinence marker present in the inflected parts of the verb. The morphonic realization of the adjective is *gambl in*; of the verb, *gambl - in*. The adjective is encoded as a single phonological word with two syllables (1-stress followed by 3-stress): [gəm blɪn]. The verb has a phonotactic realization of /gəm bl - in/ a trisyllabic phonological word, with 1-stress, 4-stress, and 3-stress. The morphological boundary marker is realized as zero in onset position of the last syllable. The hypophonemic stratum supplies such phonetically-determined characteristics as nasality on the /æ/ and /ɪ/, the continuance of the /l/ into onset position of the third syllable, etc., to give the phonetic form [gəm blɪ n̩].

The phonetic "spread" of /l/ is cited by Cygan (1971:105-8) as a troublesome problem which also applies to /r/, /y/, and /w/. But it is in fact a much more general phenomenon. Any vowel followed by a consonant has closing phonetic modifications characteristic of the succeeding consonant. Any vowel preceded by a consonant has opening phonetic modifications characteristic of the preceding consonant. A single consonant between two vowels will act as the succeeding consonant to one and the preceding consonant to the other. Thus the "spread" is a general characteristic of the phonetic continuum. If the intervening consonant is a continuant like the sonants or semi-vowels, a rigid separation line between the two syllables is not present. Hence, the spread or doubling of the sonant. This corresponds to the suggestion that a consonant may function simultaneously in two adjacent syllables.

But this is a purely phonetic matter not directly relevant to the present study. It does not complicate the phonotactic description of the "enclitic" syllable and is not counter evidence. It may therefore be ignored.

In sum, the "enclitic" syllable may optionally follow any major syllable. The sequential requirements of the "enclitic" syllable must be fulfilled, and the restrictions on accent and on the coda position of the major syllable must be met. Given these requirements, the occurrence of the "enclitic" syllable is generally automatic.

III. Neutralizations

Morphophonemic considerations are not intended to be a part of this study, but an outline of the archiphonemic neutralizations peculiar to the "enclitic" syllable is possible.

There are no special neutralizations among consonants in the final portion of the "enclitic" syllable.

In initial position there is one major neutralization in the central Connecticut dialect. The contrast between /t/ and /d/ is suspended before non-nasals if coda position of the major syllable is empty or filled by /r/, /w/, or /y/. Thus the following pairs all have homophonous second ("enclitic") syllables: *little/Biddle*, *floater/loader*, *city/Liddy*, *photo/Caddo*, *iota/coda*, *writing/riding*. This neutralization is highly sensitive to dialect, being missing in some dialects and expanded in others. Some dialects appear to have extended the neutralization of the voiced-voiceless opposition to all stops. They have *principle* with [b] and *bicycle* with [g]. I have not charted this, but it is not directly relevant here.

One other alternation occurs in initial position of the "enclitic" syllable. It is not a neutralization in the Prague sense, but it is a parallel phenomenon. The phoneme /t/ has zero realization between spirants and vocalic [ŋ]: cf. *haste* with [st] but *hasten* with [sn] or *oft* with [ft] but *often* with [fn]. There is some evidence for this realization between spirant and [l] at some time in the history of the language, cf. *castle* with [sl] < Latin *castellum*. I cannot cite a morphological example which would require such a realization in a description of contemporary English, but I suspect that a full formalization would show advantages in symmetrizing the environments of the zero realization.

Several neutralizations are possible in the vocalic part of the final portion of the "enclitic" syllable. This is also highly dialect-sensitive, but the facts for central Connecticut are easily outlined.

Two clearly phonological neutralizations involve lax vowels: /E/ and /æ/ are realized as schwa in the "enclitic" syllable. Compare the schwa in the words *competition* and *photography* with /E/ in *competitor* and /æ/ in *photograph*. For various reasons I prefer to treat the second vowel in *compete* as a morphologically determined "lengthened" or tensed variant, but that is irrelevant here.

Zero realization is necessary to account for what happens to the /I/ in *ability*, the /æ/ in *locality*, and the /a/ in *idolatry* (cf. *able*, *local*, and *idol*). This parallels the realization of /t/ in *often*.

Like the neutralizations (suspensions of contrast), the morphophonemic alternations differ from dialect to dialect. Their exact nature depends on the morphological description and is beyond the scope of this study. The morphophonemic and archiphonemic descriptions involve a wealth of dialect material, far more than space limitations permit me to describe. But the archiphonemic neutralizations in the dialect at hand have been listed.

IV. A summary of the description.

(1) Pword/((Syl·[',\'])[SylE])ⁿ

(2) SylE/Initial Final

(3) Initial/(f,s)+(t,d)+D+C

(4) Final/((i,o,ə,l,r,m,n)[z,d])+((I(C,st))[S,T])

An integrated statement of the phonotactic relations involving the "enclitic" syllable is given in (1) through (4) above. (1) states that a phonological word is related (at a lower tactic level) to a sequence of a syllable (with primary, secondary, or no accent) optionally followed by an unaccented "enclitic" syllable and taken at least once. The raised dot indicates simultaneous occurrence; the square brackets indicate optionality; the comma indicates a (tactically) free choice; and the parentheses are for grouping. A lack of punctuation indicates a left-to-right sequence. (2) states that the "enclitic" syllable is related to initial position followed by final position. The addition sign in (3) indicates preference in choice, with the most highly marked to the left. Thus (3) states that initial position may have a spirant (followed by zero realization of /t/--details not shown); otherwise it may have /d/ or /t/ if they are followed by a nasal; otherwise it will have the apical flap; otherwise it may have any other single consonant. (4) states that the final may be /i/, /o/, /ə/, or a syllabic liquid or nasal optionally followed by /z/ or /d/ (as realizations of the plural and past tense morphemes); otherwise it may have a lax /I/ followed by any single consonant or by /st/, optionally followed by the archiphonemes *S* and *T* (also realizing the plural and past morphemes).

V. Some additional considerations

English also has an optional "proclitic" syllable which consists of a schwa or of a single consonant optionally followed by a schwa or vocalic sonant. It must be followed by a major syllable with a consonantal onset. For example, contrast these pronunciations of *police*: Southern regional [pɒw li:s] vs. [pɒ li:s] and even [p^x li:s]. Compare the latter with *please* [p^xi:z], with its unvoiced [j] and aspirated onset cluster. Adding the "proclitic" syllable to (1) will allow a prediction of the full range of rhythmic possibilities in English, at least for the central Connecticut dialect.

Because of the lack of the "proclitic" syllable in the description, no materials on metric feet have been included. However, it is easy to see that many poetic feet can be predicted by (1). For example, it is possible to get a trochee in four ways: a major syllable with primary or secondary accent followed by an "enclitic" syllable or by an unaccented major syllable. Possibly an unaccented major syllable followed by an "enclitic" syllable could also be used.

These and other areas of research, e.g. variation in encoding, deserve scrutiny in the light of the "enclitic" syllable.

VI. Theoretical implications

A number of troublesome problems in English phonology have been described in a straight-forward, simple fashion in the context of the syllable. The description is biunique, fully and directly predictive of archiphonemic neutralization, zero realization, and allophonic variation. Much of the data accounted for

is described, at least implicitly, in the literature (e.g. Cohen 1965 and Cygan 1971). But an integrated description of these data has never been given.

Besides these positive advantages to the present description, there are substantial negative ones. By expounding a minor syllable type of this sort, we can avoid concocting an abstract underlying form (or "phonemic interpretation," to use a term from Bloch & Trager 1942) like /litəl/, which serves only to provide a vocalic phoneme for the second syllable or, in yet another framework, to make the rules work out. The "enclitic" syllable itself can supply the phonon 'vocalic' as a determined phonotactic feature of any syllable. Moreover, this also avoids the opposite extreme of some purely segmental phonologies, i.e. the claim that English has internal clusters (as opposed to sequences) /tɫ/ and /tn/ which are lacking in initial position (cf. Cohen 1965:62-5).

The second original contribution of the present study is the demonstration that the English syllable has subtypes with distributional properties relative to the occurrence of the major syllable which provides a phonotactic parallel to the distributional properties of the enclitic lexemes in grammar. This type of phenomenon has long been known for Mon-Khmer languages. But it seems that proving anything about the syllable in "Chinese-type" languages is considered insufficient to indicate possibilities for the syllable in Indo-European languages by those very people who are otherwise anxious to set up universal categories.

In any case, the possible functions and characteristics of the syllable have been expanded. The syllable can be given a general phonotactic description within any given language. It has, among others, been defined for Peking Chinese (Hockett 1947), Norwegian (Haugen 1956), Russian (Sullivan 1969), Hanoi Vietnamese (Sampson 1970), and English (Cygan 1971 with the present work). Except for Haugen 1956, none of the above depends on the fiction that speech has separable segments, and even Haugen's work could be recast in terms of non-segmental feature matrices. Sampson 1970 and the present work show that the syllable can be used to provide a non-arbitrary answer to the question of the structure of phonological representations. The present study shows that the syllable can define the environment for archiphonemic neutralization, and Sullivan 1976 shows that the syllable can predict the outcome of morphophonemic alternation.

Why there should be opposition to the syllable as a phonotactic construct is a mystery to me. Without making any claims it is still possible to state that there is at least as much evidence for the universality of the syllable as there is for the sentence, and more than there is for the word, both of which are more abstract and more than there is for the word. Both of these are more abstract than the syllable and more widely used as universals. I grant that some languages, e.g. Aymara, do not exploit the syllable to the extent that others, e.g. English and Chinese, do. But I have never seen a language for which the syllable is an impossibility.

However, universality is not the basic issue. With all the discussion of theoretical implications, "Ordinary Working Lin-

guists" are probably more interested in results. The studies cited above and a large body of work by the tagmemicists show that the syllable produces such results. The search for the syllable therefore ought to be a part of the descriptive aims of all phonologists.

NOTES

1. In spite of strenuous objections to the application of the term "enclitic" to phonology from men whose opinion I greatly esteem (e.g. H. A. Gleason, Jr., Kenneth Pike, and Toby Griffen), I retain it, albeit in quotation marks, for reasons which will, I hope become clear. I thank them all, especially Dr. Pike, for their time and discussion, and beg their indulgence for ignoring their advice, to wit: the term "enclitic" has a clear and unambiguous meaning in grammar; any extension to phonology can only be confusing. Please keep the usual meaning of "enclitic" in mind, as I am aiming at phonological parallels.

2. The distinction between major and minor syllable types, paralleling the one between independent and "enclitic" syllables in the present study, is traditional in descriptions of Mon-Khmer languages. I am indebted to Professor Gleason for this observation.

3. One phonetic point should be mentioned. In words like *faces* [fey siz] or *causes* [kə ziz], spirant friction is clearly audible throughout the "enclitic" syllable. This is not true of the "enclitic syllable in words like *Rosa's* [row zəz] or *posses* [pa siz]. This points in the direction of an inserted vowel. Marc Schnitzer (University of Puerto Rico) has suggested to me that this [I] vowel differs phonetically from the inserted vowel in other languages, e.g. Slavic, where the vowel inserted is a clear vowel with the archiphonemic neutralizations of other vowels in the language. English may thus have a "vowelized" consonant. I find this suggestion appealing, but do not pursue it further.

4. Professors Pike and Gleason have separately made suggestions to me about this datum. Though Pike's suggestion is in terms of wave function and Gleason's is a sequential generalization, it seems to me that they are compatible and at least partially correct. Their thrust is that the "enclitic" syllable belongs in coda position of the preceding major syllable. However, I think this suggestion fails to tell the whole story because the "enclitic" syllable is 4-stress and without pitch, whereas the major syllable it follows may have 1-stress and pitch. These prosodies normally extend over the entire syllable, being co-extensive with all positions in that syllable, including coda position. The answer must await full formalization.

REFERENCES

Bloch, Bernard, and George L. Trager. 1942. Outline of linguistics.

- tic analysis. Baltimore: Waverly Press
- Chomsky, Noam, and Morris Halle. 1968. Sound pattern of English. New York: Harper & Row
- Cohen, Antonie. 1965. The phonemes of English. The Hague: Martinus Nijhoff (reissue of a 1952 publication)
- Cygan, Jan. 1971. Aspects of English syllable structure. Wrocław, Poland: Ossolineum
- Gleason, H. A., Jr. 1955. An introduction to descriptive linguistics. New York: Holt, Rinehart & Winston
- Griffen, Toby. 1975. On describing the cluster prosody. In the First LACUS Forum, Adam Makkai and Valerie Becker Makkai (ed.), pp. 140-47. Columbia, S. C.: Hornbeam Press
- . 1978. The case against allophony. To appear in the Fourth LACUS Forum, Michel Paradis (ed.). Columbia, S. C.: Hornbeam Press
- Haugen, Einar. 1956. The syllable in linguistic description. In For Roman Jakobson, Morris Halle (ed.), pp. 213-21
- Hockett, Charles. 1947. Peking phonology. JAOS 67:253-67. Reprinted in Readings in Linguistics I:217-28
- Sampson, Geoffrey. 1970. On the need for a phonological base. Language 46.586-626
- Sullivan, William J. 1969. A stratificational description of the phonology and inflectional morphology of Russian. Unpublished Yale University Ph. D. dissertation
- . 1976. Abstractness, the syllable, and the fleeting vowel in Russian. In the Second LACUS Forum, Peter A. Reich (ed.), pp. 164-69. Columbia, S. C.: Hornbeam Press
- Trager, George L., and Henry Lee Smith, Jr. 1951. An outline of English structure. Norman, Oklahoma: Battenburg Press

THREE LEVELS OF TEMPORAL STRUCTURING IN SPOKEN LANGUAGE

Joseph Oliva and Judith Duchan
State University College at Buffalo

Consider the following sentence fragment, extracted from a conversation between two women:

(1) Like I loaned my cousin some of my stuff but...

The utterance might be described as having rhythm or rhythms, and a beat, as being delivered at a particular tempo, and some observers might group parts of the utterance into metric units. While linguists and psycholinguists would perhaps agree that spoken language has rhythm, tempo, meter, and beat, they have not agreed on how these constructs relate to one another, nor have they been wholly successful in structurally analyzing such temporal phenomena.

This paper presents a structural analysis of the temporal aspects of spoken language and in so doing it separates the four constructs of rhythm, meter, beat and tempo. It argues for three basic and interrelated structural levels: the rhythmic level where stress and duration of syllables are described; the metric level where the utterance is divided temporally into equivalent groups; and the beat and tempo level where the timing of the beats across the entire utterance originates. We will first clarify the levels and then point to evidence from normal as well as abnormal speakers which justify their existence.

The Rhythmic Level

Returning to Sentence 1, we note that the relative duration of the syllables vary. We can indicate this by adding musical notation which marks duration:

(2) Like I loaned my cousin some of my stuff but


We can see from (2) that the one-syllable word like takes the same time as the two-syllable word cousin, and the two monosyllabic words like I take more time than my stuff.

These relative durations are related to the relative stress, but not in a one-to-one way. While the longer syllables

tend to be the most stressed, one system is not predictable from the other, as can be seen in (3) where we have marked stress according to the four degrees of the Trager and Smith (1951) notation system.

(3) Like I loaned my cousin some of my stuff but

Note that I, my, but, and the first syllable of cousin were all said with primary stress, but cousin is said in a shorter time. Note also that the first my and stuff were said with tertiary stress, but my was of shorter duration. Finally, note that these relative differences could still hold even if the sentence were said at a faster or slower tempo, i.e., when the absolute durations of syllables change.

The relative duration and stress of the syllables comprise our analysis at the rhythmic level.

The Metric Level

The organization of the rhythmic level is heavily influenced by a second level of temporal structuring, the metric level.

In our now familiar fragment, there was a clearly detectible separation between I and loaned, cousin and some, and between stuff and but. Trager and Smith (1951) have notated this type of internal juncture as a single bar, on the basis of its sustained pitch. These separations divide the utterance into four units: Like I, loaned my cousin, some of my stuff, and but. While Trager and Smith (1951) called the units intonation patterns, we would like to consider them from a temporal framework and call them metric units.

When we examine the relative duration of the metric units, we can see from (4) that the first three are equivalent in duration to one another:

(4) Like I / loaned my cousin / some of my stuff / but.../

Summing over the first, we get $1/4 + 1/4 = 2/4 = 8/16$, over the second we get $3/16 + 1/16 + 3/16 + 1/16 = 8/16$, and over the third we get $3/16 + 1/16 + 2/16 + 2/16 = 8/16$. We have labeled this tendency for speakers to produce equally timed intonation

patterns across utterances isometricity (Oliva and Duchan, 1976; Duchan and Oliva, 1976).*

The metric level, then, pertains to metric units and does not include in its description the beat or tempo structure of the utterance. That, we are confining to our third level of temporal structuring.

The Beat and Tempo Level

Returning to our speaker's sentence fragment "Like I loaned my cousin some of my stuff but" consider it in terms of its beat. If we were to ask listeners to listen to the sentence and to "beat-it-out" and place upon the listeners the stipulation that they should not assign a beat to every syllable, we are likely to get a good degree of agreement. We predict that the beats would occur on I, loaned, the first syllable of cousin, on some, on my, and on but. We make these predictions on the basis of the fact that beats are most likely to be placed on the strongest stressed items. However, this is not necessarily the case. For example, if we gave more of the utterance: but...I couldn't even wear it again, we are likely to get a beat assigned to the pause between but and I. What governs the beat placement is not the strong stresses but the fact that beats tend to occur at regular intervals. That is, just like the beats in a musical line, the beats in a fluent utterance tend to be evenly spaced across the utterance.

One further structural regularity about the beat and tempo level warrants mentioning at this point, and that is that there are, in our fragment, two beats for each metric unit. This two-beat-per-meter is characteristic of all of the fluent utterances we have analyzed for adult speakers of English.

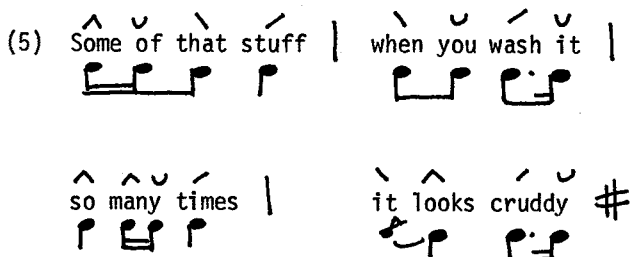
In sum, we see utterances as temporally structured from our three separate levels. We have not assumed these three levels prior to our temporal analysis, however, but rather have derived the levels from our analysis of spoken sentences. We will now present the examples which persuaded us to separate rhythm, meter, and beat and tempo from one another.

Evidence for the Beat and Tempo Level

We know from previous work done in linguistics that the strong stresses syllables in English are determined by the

*Isometricity differs from what has been called isochrony by Pike (1945) and others (Abercrombie, 1964; Allen, 1972; Anderson, 1975) in that isochrony claims a tendency for equal timing from primary stress to primary stress across an utterance, and isometricity claims it from major juncture to major juncture.

lexical, syntactic, and semantic structure of the utterance. Thus, a specific utterance might have a strong stress on any of its syllables. How can we reconcile this degree of flexibility for stress placement with the deterministic periodic placement of strong stresses to coincide with beats? We found that our normal adult speakers adjusted the duration of their syllables and that these adjustments often resulted in the stronger stresses occurring at equidistant intervals across the utterance.



In Sentence (5) we want to point out the grace note under the second it which indicates that although the word receives a tertiary stress, it is said so rapidly that one can hardly assign it a duration. Compare this to the longer word when in the second metric unit which we have assigned an eighth note duration. We believe that the speaker who is marking off two beats to a metric unit prolonged the word when because she needed it to take the first beat in the measure. However, on the last measure she had a stronger stress available, a secondary stress on looks, so she shortened the word it and used instead the stronger stressed secondary to mark the first beat in the two-beat measure.

This adjustment of duration in syllables has been reported in detail by Bolinger (1976). He offers several sentence illustrations which we will duplicate here, since the adjustments neatly substantiate the point we are trying to make.

Bolinger considers the following two sentences in parallel:

- (6) a. You make John tell who stole that calf.
b. You make the man tell who stole that calf.

He notes that the word make has a longer duration in (6a) than in (6b).

We suggest that the first meter covers You make John in Sentence (6a) and You make the man in Sentence (6b). Given that the beats are said within a fixed periodicity, make in Sentence (6a) and make the in (6b) must be said with the same duration, resulting in make being shortened in (6b).

Bolinger (1976) offers a second illustrative pair of sentences:

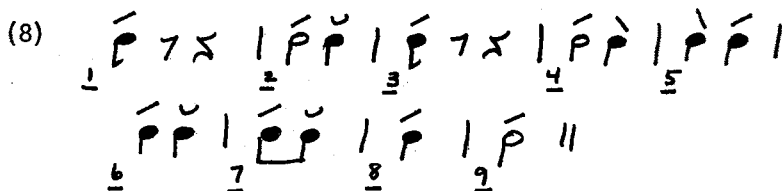
- (7) a. You make John tell who stole that calf.
b. You make John tell who it was.

In (7a) who has a longer duration than in (7b), according to Bolinger. We agree and attribute the difference to the governing beat which in (7a) falls on tell and stole, and in (7b) falls on tell and was. The result is that who in (7a) and who it in (7b) are of equal duration, producing a necessary shortening of who in (7b).

In sum, we found that our and Bolinger's observations of normally spoken sentences that speakers make adjustments in syllable duration, and we have argued that these adjustments are often made in order to have stronger stresses coincide with the governing beat. We now want to point to some final evidence for a beat and tempo level of structuring by showing what a spoken sentence might sound like without the beat structure.

We have studied the timing of the speech of a deaf 10 year old, Donna. Donna produces long strings of syllables which are separated every so often by single bar junctures. The strings, though unintelligible, give the appearance of sentence-like units, since they are intonationally cohesive and her accompanying nonverbal gestures are continuous--i.e., she does not drop her hands until the end of the string.

The timing and stresses of one of Donna's unintelligible utterances can be seen in (8):



If we take the primaries as one of the indicators of beats, we can see that the distance from one primary to the next varies considerably across the utterance so as to disallow equivalent distance between beats. The interval between beats for metric unit 2 to 3 is three times as long as the interval between beats between metric units 7 and 8, and between 8 and 9. If we were to hypothesize beats occurring regularly at the smallest interval of primary stress separation, at quarter note intervals, in this case, we would expect to see more adjustments to reflect this interval pattern. For example, the rests could have been quarter

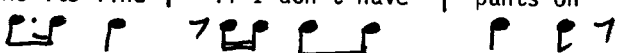
note equivalency, or the syllables of longer duration might have been considerably speeded up rather than taking as long as the half note equivalency.

Whether or not it is possible to discover a beat unit which would fit Donna's long utterance, it must be acknowledged that the beat is not as readily apparent as are those in our normal samplings and we therefore feel that Donna's speech illustrates more clearly the existence of beat structuring in fluently spoken English sentences. We would therefore characterize Donna's speech being deviant at the beat level and take it to be further evidence that one way normal utterances are organized temporally is that their stronger stresses conform to a periodic uniformly spaced beat.

Evidence for the Metric Level

We have argued so far that the beats are equidistant across the utterance. We would also like to show evidence that they are most often broken into equally timed metric units with each unit containing two beats.

The two-beat, equally timed intonation patterns, or isometric aspects of the patterning, can be illustrated by trying to make a spoken two-beat sentence conform to a three-beat meter.

(9) $\hat{N}o \ \grave{i}ts \ \acute{f}ine \ | \ \breve{y}f \ \breve{I} \ \acute{d}on't \ \grave{h}ave \ | \ \acute{p}ants \ \grave{o}n \ ||$


In Sentence (9) we believe the speaker places the beats on no and fine in the first intonation pattern; on the rest just before if and on don't in the second pattern; and on pants and on in the third.

If we were to consider the second metric unit alone we would be able to assign it a rapid three-beat structure by placing the first beat before if, second beat on don't and the third after have. However, the speaker fails to pause sufficiently after have to warrant the equidistant placement of a third beat and instead starts a new intonation unit. Furthermore, the stressing of fine would lead to a beat in the first pattern which is too close to the first beat in the second pattern to fulfill the equidistant requirement of beat structuring across intonation patterns.

Again as substantiation of the normalcy of two beats in spoken English we searched for a speaker who had a periodic beat other than two, and found it in the spoken utterances of a 2 1/2 year old normal child, Laura. Typical of Laura's utterances is the following:

(10) Sissy I / see a ocean / in airplane #

Laura starts with a two-beat then moves without breaking the periodicity into a three-beat pattern. Had this utterance been spoken by an adult in a two-beat metric, the adult might have converted the relative duration to something like (11) where the duration between the two syllables of ocean and airplane fall within a single beat rather than the two beat for each syllable in Laura's sentence.

(11) Sissy I / see a ocean / in airplane #

Thus far we have argued for two interrelated sources which determine the timing of elements in an utterance: the beat and tempo level where the overall speed and basic periodicity of the utterances are determined, and the metric level which groups the periodic beats into temporal subunits. We conceive of these two levels as working together with the metric level acting on the output of the beat level so as to organize the already defined beat structure. We see the third level, the rhythmic level acting on the output of the other two levels, adjusting syllable duration and relative stress to conform to their dictates. That is, the isometric divisions based on the two-beat structure divide the utterance into rhythmic groups which differ in syllable stress and duration.

The Rhythmic Level

Unlike for music or nursery rhymes, English utterances are not constrained by dictates from the rhythmic level. Speakers need not maintain the same rhythmic patterning across the metric units of an utterance. Indeed, if a speaker were to do this, his or her speech would sound abnormal. We reported on such a speaker at the 1975 LACUS meeting who displayed what we called isorhythmicity in his speech. This fifteen year old, Joey, violated the linguistic dictates of stress placement in order to maintain a basic rhythm across his utterances. For example, he said:

(12) Last June # to eight // to thirty #

The isorhythmicity displayed in Joey's utterance obeys the beat and isometric dictates of the other two levels, while violating the dictates of the linguistic system. Thus, we don't find in

Joey's speech the same degree of syllable duration adjustment that normal speakers exhibit. He simply marks the stronger syllables according to the overall rhythm and usually does not worry about the roving stresses from the lexical, syntactic, and semantic parts of his language.

We feel that the three-level analysis of temporal structuring in spoken language could serve to explain and describe a myriad of diverse phenomena. As we have indicated above, it offers a possibility for explaining various phonological phenomena such as unequal duration of syllables and pauses within utterances; it also provides a classification system for describing the temporal irregularities of abnormal speakers. Further, we feel that the timing mechanisms of the speaker can be felt by the listener and that this fact has the potential for explaining how listeners are able to synchronize their movement with speakers (Condon and Ogston, 1967; Kendon, 1972), and how the listeners are able to know when the speaker is signaling for a turn-taking exchange. But these latter are speculations and are as yet only indicating of our hopefulness about the implications of our findings.

REFERENCES

- Abercrombie, D. Syllable quantity and enclitics in English. In D. Abercrombie, et al (Eds.) In Honor of Daniel Jones, London: Longmans, 1964.
- Allen, G. The location of rhythmic stress beats in English: An experimental study. Language and Speech, 15, 1972, 179-195.
- Anderson, S. Ballistic control of rhythmic articulatory movements in natural speech. In D. Anderson and R. Rieber (Eds.) Developmental psycholinguistics and communication disorders. New York: New York Academy of Sciences, 1975, 236-243.
- Bolinger, D. Length, vowel, juncture. Bilingual Review, 3, 1976, 43-61.
- Condon, W. and Ogston, W. A segmentation of behavior. Journal of Psychiatric Research, 5, 1967, 221-235.
- Duchan, J. and Oliva, J. The relationship between intonation and syntax in abnormal speakers. ERIC Microfiche Collection, ED 121 069, November, 1975.
- Kendon, A. Some relationships between body motion and speech. In A. Seligman and B. Pope (Eds.) Studies in dyadic communication. New York: Pergamon, 1972.
- Oliva, J. and Duchan, J. Structural regularities in the intonation of an abnormal speaker: Psycholinguistic implications. In P. Reich (Ed.) Forum, Columbia, South Carolina: Hornbeam Press, 1976.
- Pike, K. The intonation of American English, Ann Arbor: The University of Michigan Press, 1945.
- Trager, G. and Smith, H. Outline of English structure, Studies in Linguistics, No. 3. Norman, Oklahoma: Battenburg Press, 1951.

GENERATIVE APPROACHES TO ENGLISH
PROSODY: ASSESSMENT AND OUTLOOK

Mervin Barnes and Helmut Esau
(University of Oklahoma) (Texas A&M University)

Two years before the appearance of The Sound Pattern of English, generative linguists began to turn their attention to the study of English prosody. During the ten years from 1966 to 1975, three major proposals were advanced to formalize a theory of English metrics: Halle and Keyser (1966), Magnuson and Ryder (1970), and Kiparsky (1975). The three proposals resemble each other in that they include a rule system designed to generate all and only the metrical lines of English poetry and exclude all unmetrical lines, with the qualification that Magnuson and Ryder substitute the categories occurring/non-occurring for the dichotomy metrical/unmetrical. But the three theories differ radically in their specific rule proposals. They also disagree in a number of underlying assumptions about the relationship between the abstract metrical rhythms and the language-based stress patterns.

In this paper, we wish to evaluate the adequacy of the three proposals as explanatory theories of English metrical practice. Our discussion consists of three parts. First, we sketch the main outline of the three theories. Next, we compare the different claims they make and demonstrate the inadequacy of each theory as now formulated. Finally, we suggest some directions for future investigations that could lead to a more adequate theory of English prosody.

1.1 Halle and Keyser's Theory of Prosody

Halle and Keyser's theory of the iambic pentameter has undergone a number of modifications since its first formulation. According to their (1971a) exposition, a theory of English meter consists of an abstract metrical pattern and a set of correspondence rules.

(1) The abstract metrical pattern is viewed as an alternating sequence of weak and strong positions as follows:

(W) *SWSWSWS (X) (X) where elements enclosed in parentheses may be omitted and where each X position may be occupied only by an unstressed syllable. If the optional weak position is unrealized, we obtain a headless iambic pentameter line. The two optional unstressed syllables are needed to account for the relatively frequent occurrence of extrametrical syllables at the end of a line.

According to Halle and Keyser (1966), when the poet writes a line of a particular metrical type, he '... does not violate the linguistic givens of his language but rather incorporates them into a metrical pattern which, while extra-linguistic in that the pattern is not a fact of the spoken language, is nonetheless

perfectly compatible with the linguistic givens of the spoken language'. (p. 188) To account for the fact that the same abstract metrical pattern has a large number of greatly varying realizations that must be deemed metrical, Halle and Keyser set up a basic metrical principle, the Stress Maximum (SM), and formulate a set of correspondence rules; together, these devices are intended to define precisely the set of all metrical iambic pentameter lines in English poetry. The Stress Maximum concept is defined as follows:

- (2) When a fully stressed syllable occurs between two unstressed syllables in the same syntactic constituent within a line of verse, this syllable is called a "stress maximum". (Halle and Keyser, 1971a: 169)

According to this definition, stress maxima are not possible at the beginning and the end of a line, since in these positions a syllable cannot occur between two other syllables. A stress maximum is also impossible at a major syntactic boundary. With these limitations, Halle and Keyser can account for the frequent first-foot inversions in iambic verse as well as for inversions following major syntactic boundaries, as illustrated in (3) and (4) respectively:

- (3) Lóving offénders, thus will I excuse ye (Shakespeare, Sonnet 42)
 (4) When that the póor have críed, Céasar hath wépt (Shakespeare, JC 3.2.96)

The following correspondence rules define the possible linguistic realizations of the abstract metrical pattern (1):

- (5) Correspondence Rules
 (i) A position (S, W, or X) corresponds to a single syllable OR
 to a sonorant sequence incorporating at most two vowels (immediately adjoining or separated by a sonorant consonant)
 (ii) Fully stressed syllables occur in S positions only and in all S positions OR
 Fully stressed syllables occur in S positions only but not in all S positions OR
 Stress maxima occur in S positions only but not in all S positions.

These correspondence rules are intended to establish a scale of metricality, corresponding to what is sometimes called 'degrees of metrical tension'. Rule (5i) states the principles that govern the alignment of metrical positions with syllables; rule (5ii) states the conditions for the distribution of stressed and unstressed syllables in a line. By a 'fully stressed syllable', Halle and Keyser mean the 'metrically significant stress ... borne only by major category words, i.e., by nouns, verbs, adjectives, nonclitic adverbs ... and verbal particles.... All other classes of words are metrically unstressed' (1971b: 157). Secondary and tertiary stresses are not metrically significant.

In applying the correspondence rules, we begin with the first option in each case and move down to the second and third options

if necessary, thus increasing the metrical tension of the line. Lines (6), (7), and (8) illustrate the application of the correspondence rules:

(6) The cúrfew tólls the knéll of párting dáy [53]
 W S W S W S W S W S

(7) And léaves the wórld to dárkness and to me [54]
 W S W S W S W S W S

(8) Báttér my héart, thréé-pérson'd Gód, for you [55]
W S W S W S W S W S

Line (6) satisfied both the first option of (5i) and the first option of (5ii). It is, therefore, an example of an iambic line with the highest degree of metricality. Line (7) violates the first option of (5ii) twice, viz. in the fourth and fifth S positions. However, both of these violations are accounted for by the second option of (5ii). Line (8) violates the first alternative of (5ii) four times, in positions 1, 2, 5, and 10. Two of these, viz. positions 2 and 10, become metrical under option two. The first and third W positions violate options one and two, but are allowed by option three since neither the first syllable of batter nor the word three constitute instances of stress maxima.

1.2 Magnuson and Ryder's counterproposal

Magnuson and Ryder (1970) assume that

... poetic meters are abstract patterns having no actual relation to the natural patterns of language. Meters place certain demands upon language having to do with simple contrast and alternation which language, being differently structured, is not fully able to meet. Prosody is the study of this "impossible" relation of language to meter. More precisely, it is the study of the demands which meter imposes on the verbal material and the linguistic consequences of these demands. (p. 801)

In other words, all poetry is unmetrical, according to this view. The prosodist, therefore, sets up an 'Expectation Matrix', which represents the 'impossible' demands of the abstract metrical pattern. He then writes a set of metrical base rules for a given corpus of poetry. These base rules define the allowable deviations from the demands of the Expectation Matrix.

Magnuson and Ryder reject Halle and Keyser's idea that stress alone is a sufficient basis for an explanatory prosody of English iambic pentameter verse. Rather, they assert that 'words belonging to certain types, by reason of shared prosodic characteristics, are positionally bound relative to the metrical contrast even/odd' (1970: 796). In order to account for this in part predictable metrical behavior of words, they consider syllables as bundles of four binary features: [$\pm$ Strong], [$\pm$ Weak], [$\pm$ Word Onset], and [$\pm$ Pre-Strong]. With these four features it is theoretically possible to distinguish sixteen distinct syllable types, of which eleven are realized in English, as illustrated in (9):

(9) Alphabet

	Features				Examples
	WO	WK	ST	PS	
a	+	+	+	+	
b	+	+	+	-	<u>many</u> , <u>over</u> , <u>other</u> , <u>therefore</u>
c	+	+	-	+	<u>forgive</u> , <u>believe</u> , <u>triumphant</u>
d	+	+	-	-	<u>when</u> , <u>since</u> , <u>they</u> , <u>does</u> , <u>is</u>
e	+	-	+	+	<u>grandfather</u> , <u>love-kindling</u>
f	+	-	+	-	<u>beast</u> , <u>friendly</u> , <u>saw</u>
g	+	-	-	+	
h	+	-	-	-	
i	-	+	+	+	
j	-	+	+	-	<u>before</u> , <u>another</u>
k	-	+	-	+	<u>patriotic</u> , <u>overcome</u> , <u>counterpart</u>
l	-	+	-	-	<u>saying</u> , <u>pitiful</u>
m	-	-	+	+	<u>great-grandfather</u>
n	-	-	+	-	<u>forgotten</u> , <u>counterpart</u>
o	-	-	-	+	
p	-	-	-	-	<u>death-bed</u> , <u>landscape</u> , <u>therefore</u>

(1970: 808)

The central part of the theory is the Expectation Matrix, which we reproduce in (10):

(10)	E (Even)	O (Odd)
WO (Word Onset)	+	-
WK (Weak)	-	+
ST (Strong)	+	-
PS (Pre-Strong)	-	+

This matrix represents the ideal feature combinations for even and odd positions that 'would characterize a condition of absolute metricality' (1970: 808). Since this distribution is linguistically impossible, they formulate a set of constraints, called base rules, which state the conditions under which a given feature may occur in a 'nonaffirming' relation to the matrix. According to Magnuson and Ryder, 'the Base Rules of a metrics explain the process by which equilibrium is established in the conflict between the demands of the language, on the one hand, and the

Following we reproduce the four Base Rules which Magnuson and Ryder have formulated for Shakespeare's 42nd sonnet. In (12) we give a translation of the rules in ordinary language:²

- (12) 1. In the environment immediately after a syllable which is in an even position and which is [+PS], any feature cluster must contain the feature [+PS].
2. In the environment immediately after a syllable which is in an even position and which is both [-WO] and [+WK], any feature cluster must contain the feature [+WK].
3. In the first even position immediately following or preceding any even position which is occupied by a [+WK] syllable, a [-WO] must also be a [+ST].
4. In the first odd position immediately following an odd position which is [+ST], any feature cluster must contain the feature [-ST].

In 'Stress, Syntax, and Meter' (1975), Kiparsky proposed a theory of prosody that is based on the phonological theory of Chomsky and Halle's (1968) The Sound Pattern of English and on the distribution of word and phrase boundaries in English verse. Kiparsky views a metrical system as being characterized by the following four components:

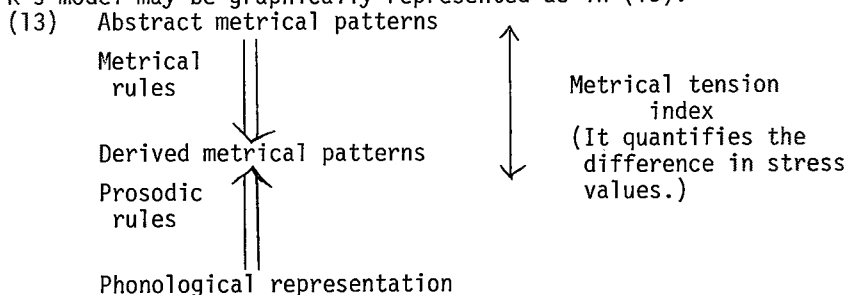
- I. An inventory of BASIC PATTERNS for the different lines; e.g., an iambic pentameter in early Shakespeare is a sequence of ten syllables, alternating between [4 stress] and [1 stress], beginning at a syntactic configuration # [# or [# , where # marks a phrase level boundary. A phrase is everything dominated by one of the syntactic categories NP, VP, AdjP, or PrepP.

- II. A set of METRICAL RULES, which take as input the basic patterns in the inventory and generate a set of derived metrical patterns. Kiparsky discusses one rule in particular, according to which a stressed syllable can replace an unstressed syllable in a monosyllabic word (the MONO-SYLLABLE CONSTRAINT) and after an intonation break (i.e., in the environment $\#_p[\# ____]$).
- III. An index of METRICAL TENSION, which defines relative metrical constraints that correspond to what is sometimes referred to as the TENSION between the abstract metrical pattern and the actual rhythm of verse.
- IV. A set of PROSODIC RULES, which specify how the derived patterns generated by the metrical rules are to be matched up with linguistic representations. This matching is not necessarily uniquely determined; hence it is necessary to state specifically what, for metrical purposes, counts as a 'stress', what counts as a 'syllable', and so on.

Of most interest here are the PROSODY RULE, according to which all stresses except the strongest stress are disregarded in each domain $\#\# X \#\#$ --where X does not contain $\#\#$ --and the so-called RHYTHM RULE, which reverses a 3-2 stress pattern before a primary stress in the next word within a phonological phrase.

(adapted from Kiparsky 1975: 580-1)

K's model may be graphically represented as in (13):



Kiparsky formulates two metrical rules, which, he says, govern the distribution of stresses in English verse. We have reproduced the rules in (14):

(14) Metrical Rule 1 (MR1): $[1 \text{ stress}] \longrightarrow [\alpha \text{ stress}]$

Metrical Rule 2 (MR2):

$[4 \text{ stress}] \longrightarrow [\beta \text{ stress}]$ in env.

$\left\{ \begin{array}{l} \# ____ \# \\ \#_p[\# ____] \end{array} \right. \quad \begin{array}{l} (a) \\ (b) \end{array}$

(The variables α and β range over the relevant values of stress (1 through 4), where 1 represents primary stress and 4 the lowest degree of stress.)

MR1 states that even positions are not constrained categorically: primary stress can be freely replaced by any other degree of stress. Only the odd position is constrained by the metrical rules. MR2 gives the two conditions under which a stress 4 can be replaced by some other stress: (a) in a monosyllabic word and

(b) after a phrase boundary. Simply stated, K is making the following claim: the metricality of a line of poetry depends solely on whether or not the odd positions--if they are stressed--are filled with monosyllabic words, unless those odd positions are immediately preceded by a phrase boundary.

2. The three theories compared

Since a full evaluation of every aspect of the three theories is not possible within the limits of this paper, we will focus here on those points that seem to us most revealing. We will proceed with our discussion under two headings: (1) ability to account for the metrical data and (2) explanatory adequacy of prosodic theories.

2.1 Ability to account for the metrical data

All three theories are unable to account for a great number of poetic lines written by our best poets. For the Halle and Keyser and the Magnuson and Ryder systems this inadequacy has been sufficiently demonstrated in the literature, most recently in Kiparsky (1975). We can, therefore, limit our discussion to a few examples. But as far as we can determine, no critical assessment of the Kiparsky theory has yet appeared in print. We are providing such an examination of the Kiparsky proposal in our forthcoming paper 'English Prosody Reconsidered'.

Although the (1971b) version of Halle and Keyser's Stress Maximum Principle is considerably weaker than the original formulation, there are still a number of constructions in English poetry which they cannot handle. Beaver (1971) and Kiparsky (1975) have correctly observed that one of Halle and Keyser's main problems stems from the fact that their metrical rules do not distinguish between compound and phrasal stress (blackbird vs. black bird). In their latest version, a syllable is either fully stressed or unstressed for metrical purposes. This means that they have two options with regard to compounds of the type blackbird: (i) both parts of the compound (i.e., black and bird) may be considered as carrying the full stresses within their respective words or (ii) the tertiary stress on the second part of compounds is disregarded as in all other cases. Kiparsky has shown that, no matter which solution is chosen, certain lines with disyllabic compounds will be considered unmetrical by the SMP.

If the first alternative is selected and disyllabic compounds are viewed as having two full stresses, lines such as (15) and (16) with disyllabic compounds in the tenth and eleventh positions will be excluded:

(15) Quite overcanopied with luscious woodbine (MND 2.2.251)

(16) Take heed of perjury, thou art on thy deathbed (Oth. 5.2.51)

Since the extrametrical syllables in (15) and (16) are not considered unstressed, they cannot distinguish between lines such as (15) and the clearly unmetrical line (17) with a fully stressed extrametrical syllable:

(17) *Quite overcanopied with luscious green vines.

However, if stress subordination is assumed, lines such as (18) and (19), with disyllabic compounds in odd-even position, will have a stress maximum in W and thus be unmetrical:

- (18) As the ^{*}death-bed whereon it must expire (Shakespeare, Sonnet 73)
 (19) In the ^{*}line-grove which weather-fends your cell (Timp. 5.1.10)

Another stress pattern that the Halle and Keyser theory cannot accomodate is the 'double trochee' or double inversion exhibited by the following line:

- (20) After ^{*}forty days fasting had remain'd (Milton, PR. II. 243)

In fact, there are many cases of simple inversion which Halle and Keyser's theory of prosody cannot handle for the same reason; witness (21) and (22):³

- (21) If I ^{*}quench thee, thou flaming minister (Oth. 5.2.8)
 (22) Whoe'er ^{*}keeps me, let my ^{*}heart be his guard. (Shakespeare, Sonnet 133)

Even though some sort of SMP is unquestionably present in stress-based meter, Halle and Keyser's formulation falls far short of explaining how it operates. However, if we were to pose the question why Shakespeare wrote iambic lines such as (23):

- (23) From hence your memory death cannot take (Sonnet 81) but not lines such as (23'):

(23') ^{*}From hence your mystical death cannot be. [construct] the SMP would be of little help to the prosodist, unless he were willing to call all such lines 'unmetrical'.

Next, we turn to Magnuson and Ryder's proposal. Since there are already several perceptive commentaries on this theory in print, we will merely give a few examples here.⁴ The main problem with Magnuson and Ryder's metrical rules is that they are too ad-hoc. Beaver is surely right when he says that

Rules are written (or discovered) for any corpus at all-- the corpus may be one line, a hundred lines, or ten thousand lines. The rules that one discovers for one sonnet of Shakespeare's may have to be rewritten when two sonnets are included, and doubtless will have to be rewritten when the 154 are taken into consideration. The rules in fact may have to be changed because of the 14th line in the 154th sonnet. (1971: 179)

In short, there is no room for generalization within this theory. It is not surprising, therefore, that Halle and Keyser (1971b: 169-170) were able to cite sets of counterexamples to every one of Magnuson and Ryder's base rules.

Let us illustrate Magnuson and Ryder's procedure with two examples. In Shakespeare's 153rd sonnet, they find pronoun + verb sequences in even-odd, with the feature composition as in (24):

- (24) If I lose thee, my loss is my love's gain.

	E	O
WO	+	+
WK	-	+
ST	+	-
PS	-	-

But the sonnet does not contain any even-odd sequences of the type illustrated in (25):

(25)	Suffr'ing	friend
	E	O
WO	-	+
WK	+	-
ST	-	+
PS	-	-

To account for the occurrence of the pattern in (24) but disallow the one in (25), they formulate the base rule (26):

$$(26) [F] \rightarrow [+WK] / \begin{matrix} -WO \\ E[+WK] \end{matrix} \underline{\hspace{1cm}}$$

After discovering the line

(27) Cupid laid by his brand and fell asleep (Sonnet 153)

they have two choices: either such lines with double inversions are labeled unmetrical or the rules have to be revised.

Similarly, all the other base rules have their problems. For example, rule (28)

$$(28) [F] \rightarrow [+PS] / \begin{matrix} \\ E[+PS] \end{matrix} \underline{\hspace{1cm}}$$

makes false predictions whenever such sequences as 'extreme fear' and 'despised love' occur in even-odd-even position, as in (29a) and (29b):

(29a) And extreme fear can neither fight nor fly (Luc. 230)

(29b) The pangs of despised love, the laws delay (Ham. 3.1. 73)

Yet such sequences occur with great frequency in English poetry.

Finally, we illustrate some exceptions to Kiparsky's prosodic theory. As mentioned earlier, the corner stone of Kiparsky's metrical theory is the monosyllabic constraint. According to this constraint, fully stressed words in odd position must be monosyllabic or the line is unmetrical as iambic verse. In Barnes and Esau (1976), we have demonstrated that the monosyllabic constraint is violated by numerous poets, including Shakespeare, the very poet whose works the constraint is supposed to characterize. Having noticed many violations of his major constraint in Donne, Kiparsky asserts that 'Donne's violations of MR2a are proof for its validity elsewhere' (1975: 605); we have, therefore, drawn our examples from those poets who supposedly do not violate it:

(30) A body so harmoniously composed,

As if nature disclosed. (Jonson, Epode: 99-100)

(31) Make not thyself a page

To that strumpet the stage; (Jonson, An Ode to Himself: 7-8)

(32) And despairs day, but for thy volume's light.

(Jonson, Dedication to Shakespeare: 8)

(33) Upon my secure hour thy uncle stole (Ham. 1.4.42.)

(34) 'Tis sweet and commendable in your nature, Hamlet.

(Ham. 1.2.87)

While the many exceptions to Kiparsky's metrical rules suggest that those rules cannot possibly be formulated accurately, Kiparsky's theory has more serious problems still. If applied to some poetry, it will completely misinterpret the meter of the poem. And what is worse still, it will accept as iambic pentameter many lines that are so patently non-poetic that no English poet would ever write them. Both of these theoretical inadequacies will be discussed in the next section.

2.2 Explanatory adequacy of three theories

In the preceding section we have demonstrated that all three theories fall far short of their stated goals. We shall now try to assess their explanatory adequacy as theories of English meter. Even though all three theories fail in different ways, they share at least two features that contribute to their failure: (1) their attempt to draw a clear-cut distinction between metrical and unmetrical lines of poetry and (2) their reliance on some non-extendable, unified principle to define what is metrical in poetry.

Although Magnuson and Ryder claim that their system distinguishes between occurring and non-occurring rather than between metrical and unmetrical lines, in practice they make the same clear-cut distinction: those lines which their base rules account for are metrical and all others are unmetrical. We doubt that this approach will lead to an explanatory theory of prosody. Rather than labeling certain kinds of inversions as unmetrical, a theory of prosody should explain why some inversions are possible and others not. Moreover, the prosodist should not try to devise a prosody that will be as valid for Keats as it is for Chaucer, but rather one that will also enable him to explain how and why the verses written by these two poets differ.

Anyone who carefully examines the English iambic pentameter tradition will recognize that the distinction between 'metrical' and 'unmetrical' verse does not remain constant from one poet to the next. Rather, we find that historically a poet either relaxes or tightens the metrical constraints used by his predecessors. The first case would seem to be an example of rule extension, while the second case constitutes rule specialization. An explanatory theory of prosody must explain how the poet can either extend or restrict the prosodic conditions of earlier poets. Yet if we understand the three theories under discussion correctly, they cannot explain that development. Their rules can simply not be extended. For example, the difference between a single trochee and a double trochee is one of extension of a principle. Thus, a prosodic theory that calls the first metrical and the second unmetrical fails completely at the level of explanatory adequacy.⁵

Secondly, in all three theories a single principle carries most of the load for defining what counts as metrical in poetry. Halle and Keyser formulate the SMP and declare it to be the major metrical principle of English poetry. Realizing that the principle is much too strong to explain the actual practice of English poets, they gradually revise the conditions for its application. By modifying the scope of its application, they hope to eventually define exactly the set of all metrical lines.

Magnuson and Ryder's prosodic theory attempts to define metricality in poetry by stating certain relations between metrical slots. The relations are a function of certain morphological and syntactic features of the words occurring in contiguous slots. To arrive at the desired distinction between metrical and unmetrical lines, the prosodist simply modifies the statement of those relations, until he can account for all occurring lines within the maximum corpus under investigation.

Kiparsky, on the other hand, makes the monosyllable constraint the corner stone of his theory. If we disregard the special case of the phrase boundary, stated as MR2b in (14), the monosyllable constraint remains as the only other substantive constraint defining the metricality of iambic pentameter lines. He then uses the combined effect of the prosodic rules, the rhythm rule, hypothetical stress changes, the phrase boundary constraint MR2b, and word boundary conventions to relax the monosyllable constraint and thereby remove many cases that would otherwise be violations.

We have seen that all three theories of prosody are unable to draw the right distinction between metrical and unmetrical lines of poetry. Much less do they provide an explanatory account of the metrical practices of English poets. The question is how much of the different theories, if anything, can be salvaged.

Let us begin with the Magnuson and Ryder proposal. Their attempt to predict metrical stress with distinctive features based on certain morphological characteristics and the lexical class membership of words seems to us totally inadequate for an understanding of the complexity of the stress patterning found both in non-metered and metered language. Recent research has clearly shown that linguistic stress does not simply operate at the lexical level but also at the phrasal and sentence level. A theory of prosody must show how these three levels of linguistic stress patterning interact to simulate a particular abstract metrical rhythm without exactly duplicating it, to create an expectancy, as it were, but to disregard that expectancy often enough to keep the poetry fresh.

The problem with defining a particular feature and then assigning it a rule within a specific slot relationship may be illustrated with the feature Word-Onset (WO). According to the Expectation Matrix, the metrical ideal is [+WO] in even and [-WO] in odd. We simply do not understand the logic of such an assertion, except perhaps as a roundabout way of saying that a large percentage of English words are trochaic. First of all, such a distinction cannot be made in the abstract. Unless the prosodist

knows how many syllables a word has, whether it is iambic or trochaic, and how it is integrated into the stress pattern of the phrase, the stated distribution of the feature makes to us no sense at all. For instance, if we are dealing with a disyllabic trochaic adjective that is preceded by an unstressed article (e.g., /the thór + / ny ...), the statement is probably accurate. But if we are dealing with a disyllabic iambic word (e.g., contént), the statement is meaningless. If other than disyllabic words are also considered, the situation becomes even more complex. In short, we do not believe that the Magnuson and Ryder proposal will lead anywhere.

We turn next to Kiparsky's theory. We must admit that Kiparsky's proposal looks very attractive on the surface. But the more we work with it, the more we become convinced that the apparent descriptive power of the monosyllable constraint is accidental. We have already shown that there are many violations of this constraint in English poetry. However, this does not mean that Kiparsky's description is completely unfounded: it is true that poets tend to avoid placing syllables with primary stress into metrically odd positions unless they also constitute words by themselves. But does that mean that poets observe a monosyllable constraint when writing iambic pentameter? We do not think so.

There seems to be a deeper reason for Kiparsky's observation. Let us take one of the many English disyllabic adjectives with a trochaic stress pattern. Two frequent syntactic arrangements into which such adjectives enter are given as (35) and (36):

(35) Det + Adj + N (e.g., his + gentle + soul)

(36) Prep + Adj + N (e.g., with + gentle + soul)

If the primary stress on the first syllable of gentle were placed into odd position, the result would be a double inversion, as illustrated in (37):

(37) ... his / géntle / sóul ...

E O E O

Now it is clear that such a double inversion is about the strongest tension that a poet will build into a pentameter line, and as might be expected, it occurs only rarely, hence the 'exceptions' to the monosyllable constraint. But by replacing gentle with a monosyllabic word, the poet can reduce the tension somewhat. In other words, what Kiparsky calls the monosyllabic constraint is one way of avoiding a clash with the expected meter of the line. But it is not the only one. Another rhythm that is avoided even in non-metered language is the spondee. It is, therefore, available to the speaker and the poet to create a contrast effect. English has two constructions that could lead to a spondee-type rhythm: compounds of the type blackbird and adjective noun sequences such as antique fables. To achieve a more regular pattern of stress alternation, the language copes with these two constructions in different ways. Compounds are 'regularized' through stress reduction on the second part, while the adjective noun sequences undergo the so-called rhythm rule, in terms of

Chomsky and Halle's system:

(38) 3 2 1 2 3 1
 antique fables → antique fables

Kiparsky has made the mistake of selecting one important device used to avoid a rhythmic clash and elevating it to the level of a general metrical principle.

It is not surprising, therefore, that Kiparsky's metrical rules make false predictions as to the meter of some poetry. In Barnes and Esau (1976) we illustrated the problem with a passage from Sir Walter Scott's 'Lochinvar', here repeated as (39):

- (39) The bride kissed the goblet; the knight took it up,
 He quaffed off the wine, and he threw down the cup.
 She looked down to blush, and she looked up to sigh,
 With a smile on her lips, and a tear in her eye.
 He took her soft hand, ere her mother could bar--
 "Now tread me a measure!" said young Lochinvar.

Kiparsky's rules predict that lines 2-4 are perfectly good iambic lines since each line is monosyllabic and may thus occur in a weak position. However, anyone reading these lines aloud will agree that they are anapaestic. There is virtually no other way to read these lines.

Not only does Kiparsky's theory make false predictions about the meter of certain lines, but we can also construct hundreds of lines that will be metrical by definition, no matter how the words are arranged. In fact, any sequence of ten one-syllable words is metrical according to Kiparsky's rules, e.g., 40:

- (40) Hack! Hack! Hack! Cut the soft, green, wet, young
 grass in strips!

A prosodic theory that has such an unrestricted metrical rule seems to us of little explanatory value.

Finally, the Halle and Keyser theory has perhaps still the most to recommend it. Something like the SMP is surely operating in poetry. But instead of viewing it as the major metrical principle of English poetry, we consider it more a guiding principle in the poet's use of the stress sequencing devices which the non-poetic language provides. The SMP is surely the most insightful element of the three theories, but it has not yet been shown that it will suffice as the principle that defines what is metrical iambic pentameter poetry. Another problem with Halle and Keyser's theory is that they concentrate too exclusively on word-level stress and disregard the counter-balancing effect of the phrasal and sentence stress patterns. As both, Wimsatt (1970) and Beaver (1971) have noted, non-lexical categories or function words 'can assume stress or slough it off, in metrical contexts ... but also in language otherwise. This stress flexibility makes these words metrically useful.' (Beaver, 1971: 189)

3. Conclusion and Outlook

Linguistic stress operates at three levels with different scopes and varying force:

- (i) lexical stress
- (ii) phonological phrase stress
- (iii) sentence stress (discourse stress?)

An adequate theory of prosody must show how these three levels of linguistic stress interact to simulate a particular abstract metrical rhythm without blindly duplicating it. The metrical theories we have examined were all found seriously lacking in this respect.

The poetic language appears to be rooted much more deeply in natural language rhythms than is often assumed by prosodists. Simply setting up a special metrical principle is unlikely to lead to an explanatory theory of prosody.

Yet when we suggest a close relationship between the language rhythms of non-metered spoken language and the prosodic principles of poetry, we do not mean that the metrical rules will proceed cumulatively in the sense that a generative grammar generates the prosodic features, beginning with the smallest unit and working toward ever larger units until the largest domain has been reached. For language rhythms are somehow primary; they exist independent of the individual prosodic rules at various levels of linguistic structure. Someone who knows the speech rhythms of a language, say English or French, can immediately identify that language from the rhythmic organization alone. The particular alignment of prosodic features is then the specific response of the grammar to the demands made by the language rhythms, which are in some sense more than the sum of its prosodic parts. The same may very well be true of the prosodic rhythms of poetry.

Yet the most important shortcoming of the current generative prosodic theories is their inability to explain the historical development of the iambic pentameter verse. The metrical rules or principles that have been proposed are simply non-extendable. Such rules are, therefore, not equipped to explain how later poets, for instance, extend the prosodic conditions of earlier poets thereby gradually modifying the boundary between metrical and unmetrical verse.

Footnotes

¹ We will not discuss here Beaver's reformulation of the Halle and Keyser theory for two reasons: (1) Beaver's theory is largely derivative from Halle and Keyser's, and (2) we are discussing Beaver's approach more specifically in Barnes and Esau (1977).

² The translation has been adopted from Halle and Keyser (1971b: 168).

³ The examples (15) - (22) have been adopted from Kiparsky (1975).

⁴See particularly Beaver (1971), Halle and Keyser (1971b), and Kiparsky (1975).

⁵We are not able to pursue the principle of extension in the present study. However, we will discuss the concept in a future paper.

References

- Barnes, Mervin and Esau, Helmut. 1976. 'English Prosody Reconsidered'. Forthcoming.
- _____. 1977. 'Gilding the Lapses in a Theory of Metrics'. Forthcoming.
- Beaver, Joseph C. 1971a. 'The rules of stress in English verse'. Lg 47.486-614.
- _____. 1971b. 'Current metrical issues'. College English 33.177-97.
- Chomsky, Noam and Morris Halle. 1968. The sound pattern of English. New York: Harper & Row.
- Halle, Morris and J. Keyser. 1966. 'Chaucer and the study of prosody'. College English 28.187-219.
- _____. 1971a. English stress: its form, its growth, and its role in verse. New York: Harper & Row.
- _____. 1971b. 'Illustration and defense of a theory of the iambic pentameter'. College English 33.154-76.
- Jespersen, Otto. 1933. 'Notes on meter'. In Linguistica, 249-74. Copenhagen: Levin & Munksgaard.
- Kiparsky, Paul. 1975. 'Stress, syntax, and meter'. Lg 51.576-616.
- Magnuson, Karl and Frank Ryder. 1970. 'The study of English prosody: an alternative proposal'. College English 31.789-820.
- _____. 1971. 'Second thoughts on English prosody'. College English 33.198-216.
- Wimsatt, W. K. 1970. 'The rule and the norm: Halle and Keyser on Chaucer's meter'. College English 31.774-88.

THE CASE AGAINST ALLOPHONY

T.D. Griffen

Southern Illinois University at Edwardsville

In segmental phonology, allophonic variation is the state in which two phones are in complementary distribution and are therefore said to be allophones of a common phoneme. As implied in its alternative terms of contextual and combinatory variation, allophony is based upon the immediate phonetic environment, and it is described in arrangement models as the appearance of a particular phone in a particular phonetic environment and in process models as the transformation of one phone (or of the base phoneme) into another as motivated by a particular phonetic environment.

For example, the initial phones of Welsh *ci* [ci:] 'dog' and its plural form *cŵn* [ku:n] are said to be allophones of the phoneme /k/. In terms of arrangement, the [c] can be described as occurring in the environment of a front vowel and the [k] in that of a back vowel (see Fynes-Clinton 1913:xxiv). By process, the phoneme /k/ can be described as transforming its underlying [+back] feature into [-back] in the environment of a vowel which is specified [-back]. In either case, the phones are predictable for backness on the basis of environment, and they are therefore considered to be allophones of a single phoneme. Moreover, in either case, variation is based upon features of a particular sound segment or opposition members inherent to a sound segment (on this point, compare Plötkin 1976).

It is this last point, the inherent nature of features or opposition members in discrete segments, which requires closer scrutiny. As pointed out by Fant (1962), whatever justification there may be for the segmental phoneme does not lie in the observable phonetic evidence, but it only exists at best in the abstract. Rather than segment-by-segment, speech is produced syllable-by-syllable (Kozhevnikov and Chistovich 1966; Mermelstein 1975), and the syllable can only be produced by synthesizing phonetic characteristics corresponding to features or opposition members in a nonsegmental relationship (see Liberman, Ingemann, Lisker, Delattre, and Cooper 1959).

In order to make the transition from the segmental phonology to the nonsegmental syllabic realization of speech, Liberman (1970) suggests the need for a model in which segments could be transformed into the nonsegmental continuum. While this concept is theoretically plausible, no workable model has ever been devised. Nonetheless, in order to maintain the transformational integrity called for by Halle (1972), Anderson (1974) is compelled to perpetuate the segment into phonology on the mere plausibility of the Liberman hypothesis.

In spite of the fact that his hypothesis is taken to be a justification for the continued use of the segment in phonetics and in phonology, Liberman reveals the basic incompatibility of this device

with the syllabic nature of pronunciation; and in so doing he provides the observation which identifies allophony as a categorical fallacy. He observes that in a CVC syllable, the vowel is produced through at least most of the syllable, and the consonants are produced simultaneously with the vowel. The hypothesis which he derives from this fact is that a transformational "grammar of speech" needs to be devised in order to account for this significant overlapping, such as that represented in figure 1. This is similar to Lockwood's stratificational shift in dominance between the sequential phonemic stratum and the hierarchical phonetic stratum (Lockwood ms.; compare also Griffen 1976b).

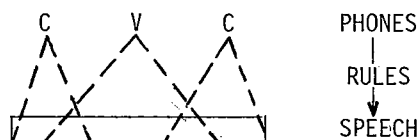


Figure 1

The problem with the Liberman hypothesis is that it assumes the correctness of segmentation and of the transformational paradigm and attempts a reconciliation between these two abstract concepts and the real observations of syllabic realization, without fully examining the implications of those observations. A closer examination of the evidence, however, indicates that the very need for this reconciliation and for such a grammar of speech lies in the fact that segmentation within the syllable has traditionally been carried out on a faulty premise, a premise still basic to the Liberman hypothesis.

This traditional segmental approach to syllable structure divides the syllable into a sequence of discrete entities, as shown in figure 2. These entities correspond to the "deep" level of figure 1. In tracing the development of these segments back through the analysis of the continuum (the "surface" level of figure 1), we can see that the segmentation of the syllable is carried out entirely with respect to time, such that the time in which the first consonant is imposed is taken to be the first segment, the time between consonants is taken to be the second segment, and the time in which the final consonant is imposed is taken to be the third segment.

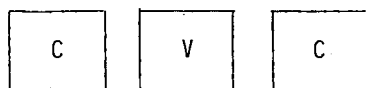


Figure 2

This approach to the consonant-vowel relationship contains the categorical fallacy in which parts of the vowel (those produced during the first and third segments) are chopped off and rather arbitrarily assigned to the consonants, precisely the problem which Liberman finds in the segmental model and seeks to solve through transformation. As a result of categorizing vowel characteristics as included in consonants, the consonant segments display not only those characteristics peculiar to the obstructions themselves, but they also display characteristics of the vowel.

To see the relationship between this categorical fallacy and allophony, let us return to the Welsh example of *ci* [ci:] and *cŵn* [ku:n]. The initial consonants differ in that the former is produced at the palatal point of articulation and with unrounded lips, while the latter is produced at the velar point of articulation and with rounded lips. As this difference is contextually (or combinatorily) determined and therefore not phonologically pertinent, [c] and [k] are considered to be allophones of the phoneme /k/. Removing the categorical fallacy of traditional segmentation, however, there is nothing inherent in these occurrences of [c] and [k], to justify considering them as variants--the difference between them is entirely in the vowel. In the one case, the vowel is palatal and marked by unrounded lips; in the other, the vowel is velar and marked by rounded lips. That the segmentalist would perceive this difference to be in the consonants is only because the consonant is produced simultaneously with the vowel, as noted by Liberman and Anderson themselves.

To cite a more extreme case, let us consider the initial consonants of the Welsh words *ffêr* [fe:r] 'ankle' and *ffo* [fo:] 'flight'. While the former is palatal and unrounded, the latter is velar and rounded. Of course, few segmentalists would argue that the two apparent variants of /f/ be allophones. Nonetheless, by the criteria establishing [c] and [k] as allophones of /k/, a strictly segmental analysis would logically classify these two occurrences of /f/ as allophonic variants because of the very same vowel characteristics incidentally included at the time of consonantal constraint.

Returning now to the hypothesis of a grammar of speech, we can see that a reconciliation between the actual syllabic realization and the segmental phone need only be made if we maintain a segmental phone. On the other hand, if we choose a nonsegmental system, such as that found in the hierarchical model (see Griffen 1976c), then we can describe the phonological relationships among opposition members within the framework of the syllable organization itself. When this is done, the need for allophony vanishes, for allophony is only a means of reconciling the observable nature of the syllable with the assumption of segmentation.

The hierarchical model of nonsegmental phonology is abstracted from dynamic phonetic evidence. As such, it is based upon the premise of "coarticulation," that consonants are constraints on vowels and are carried out simultaneously with the vowels they constrain. While no segmental model of speech has been made to work, the nonsegmental model of Mermelstein (1973) does indeed predict the articulatory movements of speech with a remarkable degree of accuracy,

just as the acoustic calculations of Öhman (1966, 1967) do in spectrographic analysis, by basing the model on coarticulation. Even Anderson (1974:5-7) recognizes this, though he fails to abstract it into his generative phonology.

When phonetic characteristics are abstracted into the phonology on the organizational basis of coarticulation rather than of segmentation, we can construct a more realistic syllable structure, as shown in figure 3. This structure makes it possible for us to assign characteristics of consonants to consonants and characteristics of vowels to vowels with no overlapping.

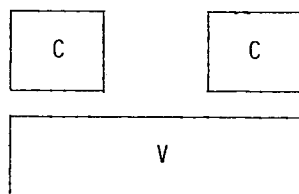


Figure 3

Considering the example of *ci* [ci:] and *cwn* [ku:n] in this new syllable structure results in an entirely different analysis. The palatal tongue position and the lack of lip rounding simultaneous with the initial consonant of *ci* [ci:] can be properly attributed to the vowel [i:]; while the velarity and roundedness simultaneous with the initial consonant of *cwn* [ku:n] can likewise be attributed to the vowel [u:]. Thus, the [c] and the [k] are not separate variants at all, but represent closure at the tongue position of the vowel within a nonpertinent range and without any effect upon the state of the lips. (For a further discussion of this relationship as it applies to the traditional problem of German [ç] and [x], see Griffen *In press*.)

In keeping with cineradiographic evidence, this syllable structure maintains a vocalic base, the slow, precise movement of the tongue-body, lips, and jaw (Mermelstein 1973:1081), extending from the beginning to the end. This is necessarily so, as obstructions must be carried out upon some sort of syllabic base, as they lack their own base of articulation; that is, they must, as obstructions, obstruct something (compare Griffen 1976c:10-11). (Of course, the tongue must be in some position, dividing the oral cavity into acoustic chambers, and the lips must also be in some state of roundness, whether or not a perceivable or pertinent vowel is discerned.)

Thus, allophonic variation of consonants based upon the position of the vowel is merely a device of segmental phonology and is not necessary in a nonsegmental system. This property of the system is supported by the findings of Perkell, who observes that "coarticulation effects of vowels are, for the most part, manifested by influencing the position of the consonant-articulating organs rather than by altering the manner of articulation" (1969:65).

Nonetheless, the manner of articulation can also be affected, as in the well-known case of Spanish "intervocalic" stops, such as the dental consonants in dama [dama] 'lady' and la dama [la ~~d~~ama] 'the lady'.

While it may be tempting to handle variation of manner as phonological alternation (compare Griffen 1977), this would only be a makeshift remedy. As demonstrated in Griffen 1976a, however, such a remedy is not necessary; for when the framework of constraint, upon which the hierarchical model is based, is used in the analysis of the physiological and acoustic event, variation of manner can indeed be attributed to the coarticulatory consonant-vowel relationship. By taking high-to-low frequency energy ratios acoustically and the behavior of the orifice of the larynx physiologically, it is shown that the lenis consonants often found in "intervocalic" environments are characterized by a lower degree of constraint, and this lower degree of constraint can certainly be attributed to the relative distribution of the vowels (as in the Spanish example). Once again, then, allophones are not needed in the system.

Shifting our attention to consonant-consonant relationships, we find that the framework of the model once more eliminates the need for allophony. As presented before the First LACUS Forum, (see Griffen 1975), oppositions of one consonant which are said to affect oppositions of another consonant are describable more simply and insightfully by prosodies, oppositions which apply without regard to segmental boundaries.

For example, the liquid in the Welsh word tros [tros] 'across' is voiceless, while that in an alternative form of the same word, dros [dros], is voiced. This apparent variation can be handled in a segmental system by a rule of assimilation according to which a liquid following a voiceless stop is voiceless and one following a voiced stop is voiced. Thus, there are two allophones [r] and [r̥]. To be sure, as shown in Griffen 1975, the segmental analysis does in fact break down on account of its segmentation when more data are considered.

A nonsegmental analysis (compare Griffen 1975) eliminates the need for allophony by the use of the obstruction prosody. Once the opposition of aspiration is freed from the artificial bounds of the segment, its devoicing effect can be maintained over an entire cluster. According to this analysis, then, there are no variants of the obstruction--it is just a case, as in the consonant-vowel relationship, of the simultaneous coarticulation of opposition members. The voicelessness of the [r̥] in tros [tros], then, is no more a basis of variation of the [r̥] itself than is the velarity and roundness of the [f] in ffo [fo:], as obstructions are constrained by prosodies in much the same manner as vowels are constrained by obstructions.

Finally, there is the case of the vowel-vowel relationship, such as that found in umlauting, or vowel affection. Perhaps the most famous example of true umlauting (of phonetic rather than of phonological pertinence) is that of Old High German. In Old High German, a word with a single back rounded root vowel and a high front vowel suffix realizes a variant of the root vowel which is

rounded but front. Thus, wurm [wurm] 'worm' has the plural form wurmi [wyrmi], unmarked in the orthography on account of its predictability. Because of this predictability on the basis of environment, a segmentalist would posit a phoneme /u/ with the allophones [u] and [y].

Again, the allophones of umlauting are also unnecessary in the nonsegmental phonology. As pointed out in Griffen 1976c:9, the vocalic pattern is continuous; it is constrained by consonants, but not broken by them. Once this property of the system is taken into account, umlauting can be handled in the same way as the consonant-consonant relationship above--by syllable prosody. According to this analysis, the distinctive marks of the root vowel in wurm [wurm] are its height and its roundedness, with backness not of phonological pertinence. The opposition of backness, then, becomes a prosody, free to extend its realization across syllables in the vocalic pattern. Not being inherent to the root vowel, backness cannot be considered as a basis for variation of the root vowel. Such a relationship of nonpertinence in the opposition of backness in this case can be compared with that of the false variation of [c] and [k], noted above.

It is hoped that a significant contribution of the hierarchical phonology can be seen in the elimination of the segment through the construction of a system based upon coarticulation rather than upon segmentation. As was demonstrated before the Third LACUS Forum (Griffen 1977), this allows us to maintain a single structure of phonological relationships, with the differences between relationships of phonological, morphophonological, and archiphonological pertinence being only functional.

It is no less significant, though, that the elimination of the phone leads to the elimination of the allophone, for the allophone is the motivation for the division between phonology and phonetics. In order to account for variants in phones due to the fallacious categorizations inherent in segmentation, structural linguists have had to create two structures, a phonological one in which there is a phoneme /k/, for example, and a phonetic one with the allophones [c] and [k].

The absence of allophony in nonsegmental phonology does more than simplify the individual relationships among oppositions. It reduces the two structures of phonology and linguistic phonetics, with their systems of interrelationships so complex as to make the Liberman hypothesis unworkable, into a single structure. In structural/functional linguistics, this represents the maximal economy of structure, as it simplifies the entire phonological system into one structure with several sets of functional relationships.

References

- Anderson, S.R. 1974. The Organization of Phonology. New York: Academic Press.
- Fant, G. 1962. 'Descriptive Analysis of the Acoustic Aspect of Speech,' *Logos* 5.2-17.
- Fynes-Clinton, O.H. 1913. The Welsh Vocabulary of the Bangor District. Oxford.
- Griffen, T.D. 1975. 'On Describing the Cluster Prosody,' The First LACUS Forum, ed. by A. Makkai, 140-7. Columbia: Hornbeam.
- Griffen, T.D. 1976a. 'An Inner-Approach Analysis of the Fortis-Lenis Scale,' paper presented before the 1976 Mid-America Linguistics Conference, October 1976, Minneapolis, Minnesota. (To appear in the Proceedings of the 1976 Mid-America Linguistics Conference)
- Griffen, T.D. 1976b. 'Stratificational Grammar and an Hierarchical Phonology,' Views on Language, ed. by R. Ordoubadian and W. Von-Raffler Engél, 245-9. Murfreesboro: Inter-University.
- Griffen, T.D. 1976c. 'Toward a Nonsegmental Phonology,' *Lingua* 40. 1-20.
- Griffen, T.D. 1977. 'Functional Relationships in an Hierarchical Phonology,' The Third LACUS Forum, ed. by R.J. DiPietro and E.L. Blansitt, Jr., 573-9. Columbia: Hornbeam.
- Griffen, T.D. In press. 'German [x],' to appear in *Lingua*.
- Halle, M. 1972. 'Theoretical Issues in Phonology in the 1970's,' Proceedings of the Seventh International Congress of Phonetic Sciences, ed. by A. Regault and R. Charbonneau, 179-205. The Hague: Mouton.
- Kozhevnikov, V.A., and L.A. Chistovich. 1966. Speech: Articulation and Perception. Revised. U.S. Department of Commerce, JPRS 30, 543.
- Liberman, A.M. 1970. 'The Grammars of Speech and Language,' *Cognitive Psychology* 1.301-23.
- Liberman, A.M., F. Ingemann, L. Lisker, P. Delattre, and F.S. Cooper. 1959. 'Minimal Rules for Synthesizing Speech,' *Journal of the Acoustical Society of America* 31.1490-9.
- Lockwood, D. ms. 'On the Nature of the Phonetic Stratal System.' Mimeo.
- Mermelstein, P. 1973. 'Articulatory Model for the Study of Speech Production,' *Journal of the Acoustical Society of America* 53. 1070-83.
- Mermelstein, P. 1975. 'Automatic Segmentation of Speech into Syllables,' *Journal of the Acoustical Society of America* 58.880-3.
- Ohman, S.E.G. 1966. 'Coarticulation in VCV Utterances: Spectrographic Measurements,' *Journal of the Acoustical Society of America* 39.151-68.
- Ohman, S.E.G. 1967. 'Numerical Model of Coarticulation,' *Journal of the Acoustical Society of America* 41.310-20.
- Perkell, J.S. 1969. Physiology of Speech Production. Cambridge: MIT.
- Plotkin, V.Y. 1976. 'Systems of Ultimate Phonological Units,' *Phonetica* 33.81-92.

AN ARGUMENT FOR PARTIAL SPECIFICATIONS:
EVIDENCE FROM SPANISH

M. Carmen Lozano
Indiana University

It is a well-known fact that most dialects of Spanish exhibit a stop-continuant alternation in the voiced obstruent series: [b] alternates with [β], [d] with [ð], and [g] with [ɣ]. In generative treatments of Spanish phonology, it has been assumed that the directionality of the alternation is from stop to continuant (cf. Harris, 1969). The purpose of this paper is to question this assumption and to propose an analysis that derives both stops and continuants from a single source that is unspecified for the feature [continuant]. This position makes explicit the claim that the feature [continuant] is not distinctive for the voiced obstruents.

In Spanish [b,d,g] and [β,ð,ɣ] are in complementary distribution. The stops occur in initial position (utterance or word and utterance initial position)¹ and after a homorganic non-continuant sonorant (i.e. after homorganic nasals and laterals); the non-strident continuants occur elsewhere. The examples in (1a) and (1b) illustrate the occurrence of the voiced stops and non-strident continuants respectively:

(1) a.	[b]	[d]	[g]
	ámbos umbéso	dónde tóldo undwénde eldwénde.	tángo ungáto
b.	[β]	[ð]	[ɣ]
V	lóβo	áoα	máyo
C	aõBenimyénto	súðõito	suβyloátal
V	aõikár	aõmirár	mayõaléna
w	ewβólya	awõitóryo	awýústo
	aβwélo	aõwána	aywero
y	seyβo	(náyðen) ²	ařayyo
	sáβya	aõyós	káyya
r	árβol	arðér	lárýo
	aβrír	aõryátiko	áyryo
l	álβa	[d]	ályo
	aβlár	aõláteres	síylo
	kluβ	atauð	siysáy

Harris (1969) assumes that the stops are basic and proposes a rule of Spirantization to account for the alternation. Harris's Spirantization rule is given here as (2).

(2) Spirantization³

$$\begin{bmatrix} +\text{obstr} \\ -\text{tense} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{cont} \\ -\text{strid} \end{bmatrix} / \left\{ \begin{bmatrix} +\text{obstr} \\ +\text{cont} \\ <[-\alpha\text{cor}]> \end{bmatrix} \right\} \underline{\hspace{1cm}} <[\alpha\text{cor}]>$$

This rule is intended as a formal statement of the following generalization: a non-tense obstruent (i.e. a voiced obstruent: b d g) becomes a non-strident continuant ($\beta \delta \gamma$) when preceded by either an obstruent or a continuant (a continuant sonorant: a vowel, a glide or the liquid [r]) or a non-homorganic non-continuant sonorant.⁴ Rule (2) accounts for the elsewhere part of the distributional statement given above.

The assumption that the stops are basic is not motivated by Harris, nor can it in principle be motivated on the basis of the Spanish data. The complementary distribution of the stops and continuants in quite specific environments makes it impossible to determine which is basic and which is derived. One could just as well assume that $\beta \delta \gamma$ are basic and account for the alternation by means of a Stop Formation rule such as (3) below.

(3) Stop Formation

$$\begin{bmatrix} +\text{obstr} \\ +\text{voice} \end{bmatrix} \rightarrow [-\text{cont}] / \left\{ \begin{array}{l} \# \underline{\hspace{1cm}} \\ \begin{bmatrix} -\text{obstr} \\ -\text{cont} \\ [\alpha\text{point art.}] \end{bmatrix} \underline{\hspace{1cm}} \end{array} \right\} \begin{bmatrix} \alpha\text{point art.} \end{bmatrix} \quad ^5$$

Rule (3) states that a voiced obstruent becomes a stop initially and after a homorganic non-continuant sonorant. To my knowledge, none of the generative analyses of Spanish that have appeared in print incorporate a rule of Stop Formation.

The reason generative linguists have assumed /b d g/ to be basic rather than $\beta \delta \gamma$ has to do with the notion of markedness—the voiced fricatives being considered more marked than the stops. Notice that if /b d g/ are assumed to be systematic phonemes of Spanish, the consonant inventory will be that given in (4).

(4)	p	t	č	k
	b	d		g
	m	n	ñ	
		l		
		r		
	f	s	x	

Under the assumption that /β ð γ/ are systematic phonemes, the inventory of phonemes would be that of (5).

(5)

p	t	ʧ	k
β	ð		γ
m	n	ɲ	
	l		
	r		
f	s		x

(4) is considered to be a more natural (less marked) phonemic inventory than (5). Thus, the "intuitive" appeal of Harris's analysis derives from the view that if there is a single series of obstruents (the voiced obstruents in the case of Spanish) these will be stops rather than continuants. Such a view is based on the work of R. Jakobson on phonological universals. In Child Language and Phonological Universals, Jakobson points out that "the acquisition of fricatives presupposes the acquisition of stops in child language" and that "...in the linguistic systems of the world the former [fricatives] cannot exist unless the latter [stops] exist as well."

Although Jakobson's statement says nothing about voiced vs. voiceless stops and fricatives, his statement has been interpreted to mean that if the phonemic inventory of a language has voiceless fricatives, it will have the corresponding voiceless stops; and if it has voiced fricatives it will have the corresponding voiced stops. If this is indeed a phonological universal, the systematic phonemic inventory given in (5) is ruled out on universal grounds and we are forced to choose a Spirantization analysis over one with Stop Formation. There is, however, an alternative consistent with the facts and which does not violate the universal, namely, to derive both stops and continuants from a single underlying representation unspecified for the feature [continuant].

In most Spanish dialects the only voiced obstruents are [b d g] and [β ð γ]. The feature [+voice] distinguishes these obstruents from the rest. The feature [continuant] is not distinctive for the voiced obstruents because voiced stops and voiced continuants never contrast--they are in complementary distribution. Thus, for the voiced obstruents the specification for the feature [continuant] is always determined by the environment. This being the case, we would like to argue, following Shibatani and Crothers (1974), that there is no motivation for a fully specified matrix, due to the impossibility of determining from the language whether the stops or the continuants are basic.

Crothers and Shibatani argue for partial specifications for cases such as nasal assimilation in many languages where the nasal invariably takes the point of articulation of the following consonant, and there is no way of determining a fully specified base form. They point out that the objections raised in the literature

(Stanley, 1967) concerning the improper use of blanks (making ternary use of binary features) and the arbitrariness involved in the choice of features to be left blank, do not arise in these cases. The features left blank--point of articulation features in the case of the nasals, and [continuant] in our Spanish example--will always be uniquely determined by the environment.

Thus, Crothers and Shibatani take the position that blanks should be allowed in certain well-defined cases, namely, those cases "where the phonetic representation of alternating morphemes are entirely determined by their environments, there being no way to determine a fully specified base form."

This is the position adopted here, which is akin to Halle's position in The Sound Pattern of Russian (1959), where he suggests that phonemic segments are to be specified only for those features that are not predictable by the phonological rules of the language. It is at this level (the phonemic level) that we represent the speaker's knowledge of what phonological contrasts are distinctive--explicitly, rather than by reference to "deducing" the distinctive contrasts from the entire lexicon and set of phonological rules, as Postal (1968) and Anderson (1974) suggest. These latter proposals have never been more than programmatic sketches for incorporating the notion of "distinctive contrast" into a post-Sound Pattern of Russian generative phonology, and these proposed complications have never been empirically shown to be necessary (pace Lightner's and Stanley's hypothetical examples).

We propose, then, an analysis for Spanish which leaves the feature [continuant] unspecified for the voiced obstruents in underlying representations and we posit the following phonological rule to account for the occurrence of the stop and continuant allophones of the voiced obstruents:

$$(6) \quad \begin{bmatrix} +\text{obstr} \\ +\text{voice} \end{bmatrix} \rightarrow \left\{ \begin{array}{l} [-\text{cont}] / \left\{ \begin{array}{l} \# \text{---} \quad (a) \\ [-\text{obstr} \\ -\text{cont} \\ \alpha\text{point art}] \text{---} [\alpha\text{point art}] \quad (b) \end{array} \right. \\ [+cont] \quad (c) \end{array} \right.$$

This rule states that a voiced obstruent will be non-continuant initially and after a homorganic non-continuant sonorant (i.e. a homorganic nasal or lateral), and continuant elsewhere. The rules abbreviated in (6) are in an elsewhere relationship and the Elsewhere Condition proposed by Kiparsky (1973) correctly predicts that they will apply disjunctively.

Thus far we have argued, on methodological grounds, that the voiced obstruents in Spanish should be left unspecified for the

feature [continuant] in underlying representations because of the impossibility of determining whether the stops or the continuants are basic, and further because the occurrence of both stops and continuants is determined by the environment. In cases of this sort partial specification of segments in underlying representations must be allowed.

A proponent of the Spirantization analysis might argue that our example does not show necessity for leaving the feature [continuant] blank in underlying representations. All we have shown, he or she might argue, is that the choice between marking these segments as [-continuant] or [+continuant] is an arbitrary one on the basis of the Spanish data, but, on the basis of phonological universals, the choice of the stops as basic is non-arbitrary.

In the next section of this paper we will show that there is independent evidence for a Stop Formation process in Spanish. If segments which are not stops underlyingly appear as stops in precisely the environment where [b d g] appear, this is certainly no coincidence, but rather constitutes evidence for a Stop Formation process in the language which affects all voiced obstruents.

On the basis of the evidence to be presented below, we will conclude that there is a Stop Formation process in Spanish which affects /ʒ/ (in Porteño Spanish) and the glides /y/ and /w/ (which become obstruents in certain environments), as well as the voiced obstruents discussed above. The position that [b d g β ð γ] are stops underlyingly (i.e. /b d g/) becomes untenable unless it can be shown that the Stop Formation process that affects /ʒ/, /y/ and /w/ is totally unrelated to the process that affects the other voiced obstruents. Since there is evidence that the stops /b d g/ are not basic, then either the continuants /β ð γ/ are basic or the voiced obstruents must be left unspecified for [continuant] in underlying representations. As pointed out above, Jakobson's universal rules out /β ð γ/ as possible underlying representations. If this phonological universal is valid, then, we have an empirical argument for leaving the feature [continuant] unspecified for the non-strident voiced obstruents in Spanish.

In Porteño Spanish, the dialect spoken in Buenos Aires (Argentina), [b d g] and [β ð γ] are not the only voiced obstruents. The voiced strident continuant [ʒ] also occurs. [ʒ] appears initially, finally, and medially except when preceded by a homorganic nasal or lateral.⁶ In this position the corresponding affricate [dʒ] always occurs. The examples in (7) illustrate the occurrence of [ʒ] and [dʒ].

There is additional evidence for an analysis that incorporates rule (8b). Not only do basic /ʒ/'s undergo the postulated Stop Formation rule ((8b)), but also derived ones. In *Porteño* there are alternations of the form $y \sim ʒ$, $i \sim ye$ and $i \sim ʒe$. Consider the examples given under (9) below.

(9)	a.	$y \sim ʒ$	b.	$i \sim ye$, $i \sim ʒe$
		(Singular - Plural)		(Infinitive - Pres. Part.)
	ley	léʒes	biβír	biβyéndo
	bwey	bwéʒes	saíír	salyéndo
	rey	reʒes	ir	ʒéndo
	kombóy	kombóʒes	uír	uʒéndo
			-r:	Infinitive ending
			-ndo:	Present Participle ending

The examples in (9a) show that /y/ is strengthened to [ʒ] in syllable initial position. (Since both [y] and [ʒ] can occur in final position--compare (9a) with examples given in (7)--the directionality of the alternation could not be from /ʒ/ to [y].) In (9b) we give examples of verbs with basic /i/'s (the theme vowel for 3rd conjugation verbs), which diphthongize in certain morphological environments. The output of the diphthongization rule, the diphthong /ye/ is subject to the same rule that accounts for the alternations in (9a), the rule that changes /y/ to /ʒ/ in syllable initial position.⁹ Furthermore, a form like [ʒéndo] will invariably occur with an affricate instead of a fricative when preceded by a homorganic nasal or lateral, as in [aúndʒéndo] ("even going") and [manwéɪdʒéndo] ("Manuel going").

If a proponent of Spirantization assumed that the affricate /dʒ/ rather than the fricative /ʒ/ is underlying, he would still need a rule like (8b) to account for the alternations discussed above, i.e. he would need to posit a rule that changes derived /ʒ/'s to stops (i.e. affricates) when preceded by homorganic non-continuant sonorants. This would be in addition to a Spirantization rule.

There is still further evidence for a Stop Formation process in *Porteño*. This derives from the behavior of the glides /y/ and /w/, which become obstruents in initial position, as noted by Harris (1969) for Mexican Spanish. This rule is given as (10) below:

$$(10) \quad \begin{bmatrix} -\text{voc} \\ -\text{cons} \end{bmatrix} \rightarrow [+cons] / \# ______$$

Thus forms such as "hielo" [ýélo], "hiena" [ýéna], "hierro" [ýéro], "hiato" [ýáto], and "huevo" [wéβo], "hueso" [wéso], "huel-la" [wéʒa] are pronounced with initial voiced continuant obstruents in *Porteño* and other dialects. Although the obstruents

[ǃ] and [ǂ] do not become stops initially, they do turn into the stops [dǃ] (affricate) and [gʷ], respectively, when preceded by a homorganic nasal or lateral. Recall that this is the position where [b d g] and [dʒ] occur. The examples in (11) illustrate the alternations [ǃ] ~ [dǃ] and [ǂ] ~ [gʷ]:

(11)	[ǃ]	[dǃ]
	ǃéðras	soñdǃéðras
	ǃénas	soñdǃénas
	ǃáto	eʌdǃáto
	ǃéfo	eʌdǃéfo
	ǃélo	koñdǃélo
	[ǂ]	[gʷ]
	ǂéso	ungʷéso
	ǂéβos	kongʷéβos
	ǂéhpeðes	songʷéhpeðes
	ǂérfano	ungʷérfano

ñ: palatal nasal

ʌ: palatal lateral

The failure of [ǃ] and [ǂ] to become stops initially could perhaps be attributed to their strident nature (i.e. they pattern phonologically with the strident [ʒ]) or to their derived obstruent status.¹⁰ Be that as it may, these derived obstruents invariably appear as stops in precisely the environment where all other voiced obstruents appear as stops: after a homorganic non-continuant sonorant. It is our contention that the same phonological rule that derives [b d g] and [dʒ] in this environment, namely, rule (8b), derives the stop allophones of /y/ and /w/.

In the case of the glides, it would be difficult to argue that they are underlying stops. It has been argued by Harris (1969) that some of these phonetic obstruents are reflexes of underlying vowels, namely, /e/ and /o/, which diphthongize in certain morphological environments producing ye and we. On the basis of alternations such as [ǃélo] ~ [elaðo] ~ [elaðera] and [ǂéβo] ~ [oβaryo] ~ [oβiforme], Harris posits underlying forms with diphthongizing /e/ and /o/, respectively. Thus, the underlying representation for "huevo" is /obo/ and that for "hielo" is /elo/, according to Harris. The stressed é and ó become diphthongs (giving ye and we), the consonantization rule ((10)) turns the glides into obstruents—giving [ǂéβo] [ǃélo]. At this point if a nasal or lateral precedes the obstruent it will assimilate in point of articulation to it, and in Porteoño rule (8b) will apply to produce forms such as those in the second column of (11). Even if Harris's analysis turns out to be incorrect, there does not seem to be any evidence that points to an underlying stop in forms such as "hielo" and "huevo". They

either have underlying initial /e/ and /o/ or /ye/ and /we/ respectively.

Conclusions

In this paper we have shown that the voiced strident continuant /ʒ/ occurs as a non-continuant (affricate) [dʒ], as well as that /y/ and /w/ occur as the non-continuant [dʝ] and [gʷ] in precisely the environment where [b d g] occur. Our analysis claims the rule involved in the derivation of [b d g] after homorganic non-continuant sonorants is the same rule that derives the stop allophones of /ʒ/, /y/ and /w/. The phonological behavior of these segments provides strong evidence against the assumption that /b d g/ are basic. In our analysis, Stop Formation and Spirantization, rule (8), are the two sides of a single coin. It is a unitary process that spells out the continuant or non-continuant nature of the voiced obstruents, in complementary environments.

The evidence presented indicates that the stops /b d g/ cannot be basic, but it does not rule out positing the continuants /β ð ɣ/ as basic. However, such a proposal would violate Jakobson's phonological universal. If the universal is valid, then the only possible solution is the one proposed here. In sum, on the basis of the evidence presented and to the extent that Jakobson's universal is valid, we have an empirical argument for partially specified segments in underlying representations.

Footnotes

- ¹ In slow careful speech, the voiced stops occur word initially as well as utterance initially, but in fast speech they occur only in utterance initial position. After a homorganic non-continuant sonorant, the stops always occur, regardless of style of speech. (cf. slow speech: [dorotéa béβe ɣindáðo] fast speech: [dorotéa βéβe ɣindáðo])
- ² [náɣðen] is a non-standard form cited by Harris (1969). I have been unable to find other examples of [ð] after [ɣ].
- ³ John Goldsmith (personal communication) points out that the use of angled brackets in rule (2) does not do what it is clearly intended to do. Rule-schema (2) as stated includes, as its final expansion (when the angled brackets are removed), a context-free change to [-continuant]. How or whether Harris' intended generalization could be formalized remains unclear. Our version, (6) below, does not have such a formal difficulty.
- ⁴ The non-continuant sonorants in Spanish are the nasals and laterals. The condition that Harris tries to express with the use of angled brackets in the third expansion of rule (2),

namely, disagreement in coronality, is intended to ensure the occurrence of [β] and [ð] after the non-continuant sonorant [l] as in [álβa] and [álðo], as well as the non-occurrence of [ð] after [l], since in this position only [d] occurs.

- 5 We are using [point art] (i.e. [point of articulation]) as an abbreviation for all features that indicate point of articulation. Thus, [αpoint art] abbreviates

αanterior
βcoronal
ðback
γdistributed
- 6 In the speech of some Porteño speakers, [ʒ] and [dʒ] seem to be free variants in initial position. In my own speech [dʒ] rarely occurs in this position.
- 7 We have been assuming all along that Spanish has rules of nasal and lateral assimilation. For a discussion of these rules, see Harris (1969)
- 8 It seems possible to consider rule (8a) to be a variable rule which affects different kinds of obstruents to varying degrees. The variability of its application seems to correlate with the different kinds of voiced obstruents. It applies more often (or perhaps, always) to non-strident voiced obstruents, less often to a strident voiced obstruent (see footnote 6), and not at all to the derived voiced obstruents $\tilde{y}$ and $\tilde{w}$ (at least in Porteño). It has been reported in the literature (cf. Malmberg 1950) that the continuants [β ð γ] occur in initial position in relaxed casual speech. This may mean that for some styles rule (8a) is optional, or that it is not a rule of rapid speech at all. Given our analysis, if rule (8a) is optional, whenever it does not apply, rule (8c) will automatically apply.
- 9 This strengthening rule is a minor rule, since not all syllable initial /y/'s become [ʒ]'s. The distinction between [ŷ] and [ʒ] made in this dialect is reflected in the orthography, and no doubt had its origin in it. The orthographic symbols "ll" and "y" represent [ʒ], while "hi" represents [ŷ]. This distinction, though artificial in origin, has been fully incorporated into the grammar. Speakers never hesitate or "make mistakes" when pronouncing words such as "hiena" [ŷéna] vs. "llena" [žéna].
- 10 [ŷ] and [w̃] are clearly fricative consonants, however, their strident nature is questionable. Stridency is a feature that is defined relative to other sounds, and it is, therefore, difficult to determine phonetically whether a given sound is strident or not. [ŷ] and [w̃] clearly pattern with the voiced obstruents in a position after a nasal or a lateral. Their failure to become stops in initial position does not weaken

our argument. Whether we accept the position taken here or not, the occurrence of the stop allophones after a homorganic nasal or lateral, as well as the occurrence of the continuant allophones in initial position, will have to be accounted for somehow.

We are assuming the following feature specifications for the glides, /ʝ/, and their allophones:

	y	ÿ	dÿ	ž	dž	w	ŵ	g ^w
vocalic	-	-	-	-	-	-	-	-
consonantal	-	+	+	+	+	-	+	+
anterior	-	-	-	-	-	-	-	-
coronal	-	-	-	+	+	-	-	-
high	+	+	+	+	+	+	+	+
back	-	-	-	-	-	+	+	+
round	-	-	-	-	-	+	+	+
continuant	+	+	-	+	-	+	+	-
voice	+	+	+	+	+	+	+	+

Bibliography

- Alarcos Llorach, A. 1964. Fonología Española. Editorial Gredos, Madrid.
- Anderson, Stephen R. 1974. The Organization of Phonology. Academic Press, New York.
- Halle, Morris. 1959. The Sound Pattern of Russian. The Hague: Mouton.
- Harris, James W. 1969. Spanish Phonology. MIT Press, Cambridge, Massachusetts.
- Jakobson, R. 1941/68. Child Language, Aphasia, and Phonological Universals. The Hague: Mouton.
- Kiparsky, Paul. 1973. "'Elsewhere' in phonology." In A. Anderson & P. Kiparsky (Eds.) A Festschrift to Morris Halle. Holt, Rinehart and Winston, New York.
- Postal, Paul M. 1968. Aspects of Phonological Theory. Harper and Row, New York.
- Stanley, Richard. 1967. "Redundancy Rules in Phonology." Language 43:393-436.

A DIFFERENT PERSPECTIVE TOWARD: "ECONOMY
AND SYMMETRY IN ENGLISH CONSONANTS"

Melvin J. Hoffman
State University College at Buffalo

The second LACUS forum provided an opportunity to hear the paper whose title is set off within the above title. (Gleason, 1976) working within the framework of stratificational phonology reexamined traditional treatments of the English consonant system.

The traditional treatments, see figure one, tend toward great assymetry when phonetic specifics are emphasized. (Gleason: 211) intended to: "shift the weightage toward phonological considerations seeking greater symmetry and economy, requiring only that phonetic facts must not be contradicted."

I have no intention to restate every detail of his argument, but some general principles are clear. For example, phonological systemic considerations may prompt the analyst to collapse phonetically distinct categories of articulation or manner. Examples of such collapsing in English articulations would be the bilabial and labial columns as well as the dental and alveolar. The manner counterpart would be the collapsing of groove and slit spirants as well as the stop and affricate rows. Continuing such a process leads to an arrangement as in figure two.

(Gleason: 211-2) makes a convincing case that sufficient phonetic grounds exist to permit a phonological distinction between two sub-categories of apical. Although dental and alveolar collapse, Gleason maintains that [r], [z], and [s] require separate categorization. He does not name the new category but designates it: Rz.

Gleason eliminates the resonant category, suggesting that [w], [l], [r] and [y] embody a single articulatory feature respectively. These features are: labial Lb, apical Ap, Rz and frontal Fr. Stops and affricates fill the single category: closed Cl. Dorsal Do, spirant Sp and nasal Ns retain the familiar values.

Figure One

	Bila- bials	Labio- dentals	Den- tals	Alveolars	Prepa- latals	Ve- lars
Simple	b p			d t		g k
Affri- cated					ʃ ʧ	
Flat		v f	ð θ			
Grooved				z s	ʒ ʂ	
Nasal	m			n		ŋ
Lateral				l		
Median				r		
Semi- vowel	w				y	

Figure Two

	Labials	Apicals	Frontals	Dorsals
Stops	b p	d t	ʃ ʧ	g k
Spi- rants	v f	ð θ z s	ʒ ʂ	
Nasals	m	n		ŋ
Reso- nants	w	l r	y	

Figure Three

	<u>Lb</u>	<u>Ap</u>	<u>Rz</u>	<u>Fr</u>	<u>Do</u>	(9 features)
<u>Cl</u>	b	d	(1)	ʃ	g	h <u>Vl</u>
<u>Sp</u>	v	ð	z	ʒ	(4)	
<u>Ns</u>	m	n	(2)	(3)	ŋ	
	w	l	r	y	(5)	

The consonant [h] is considered a realization of the single feature: voicelessness Vl and outside the matrix. In addition, every voiceless consonant is phonemically analyzed as a cluster with [h]. Consonants [p], [t], [č] and [k]--for example--are considered phoneme clusters i.e. /hb/, /hd/, /hǰ/ and /hg/ respectively. See similar reasoning in (Lamb, 1966). This results in the following matrix in figure three. The numbers in parentheses mark empty positions which Gleason fills.

Gleason spends the major portion of his article filling empty positions with phonetic entities. He fills the first position noting that [tr] and [dr] are affricates parallel to [č] and [ǰ]. He fills the second and third positions with the partially assimilated nasals in words such as "onion" and "barn" respectively. He fills the fourth position with paralinguistic segregates (Trager 1958: 6-7) and some speakers' partially aspirated final velars. The fifth position holds the velar lateral [L].

What disturbs me is that his presentation implies, for these five phones, status equivalent to phones conventionally assigned phonemic status. Gleason (215) spends several paragraphs decrying the "tyranny of segments." Obviously, he finds stratificational insights a liberating influence.

Unintentionally or intentionally, however, a covert premise lurks in various remarks. The premise is: that new insights obviate old insights as a matter of course. This can happen, but new insights can also substantiate old concepts and sometimes with even greater rigor.

It is not my intention to be either presumptuously or perversely critical of Gleason's presentation--actually the reverse. He quite clearly states (215) that he was chiefly interested in illustrating how a change of framework accomplishes several valuable aims. It "changes questions more easily asked, gives different weightage to 'facts', changes perspectives." The definitive analysis of English consonants did not interest him as greatly as: "presenting ideas worth exploring."

Nonetheless, I am uncomfortable with apparent unnecessary neglect of older, useful analytical criteria such as contrast and distribution. Taking Gleason's procedures to their logical conclusions creates more problems than solutions. I do not intend to question his data--only his interpretations.

For example, [ts]--in some positions--is as much an affricate phonetically as [ts]/ç/ or [dr]. Does this present a genuine challenge to the conventional contrastive representation /ts/ or Gleason's /hdz/? Likewise, under certain conditions, /nt/ in some speakers' dialects occur as a flapped [n']. Must an analyst seek a new unary feature such as flapping and reject the idea of a portmanteau phone?

Although I am not a stratificationalist, I find great merit in unary specification. The necessity to maintain a phonetic foundation, while not neglecting systemic symmetry and economy from phonological considerations, is certainly important. However, I do not see unary feature specification as necessarily in conflict with earlier treatments. I intend a presentation in harmony with both.

I use nine features like Gleason. (Gleason's article states ten, but I count only nine.) I differ in three respects however:

1. Three features rather than two are necessary to specify some phonemes.
2. For systemic reasons, I merge Rz and Do and represent the merger as RD.
3. For reasons outside the scope of this paper, I am unconvinced that [w, r and y] are single feature realizations. I prefer to assign them a feature of resonant Rs.

I agree that /h/ is outside the matrix however. I do not intend to discuss the /h/ here. It requires a fairly long discussion by itself. The features other than RD and Rs, are the same as those listed by Gleason. They are: labial Lb, apical Ap, frontal Fr, closed Cl, spirant Sp, nasal Ns, and voiceless Vl.

My presentation in figure four divides these nine features into three groups: articulation, manner and modification. The articulatory categories are: Lb, Ap and RD. The manner categories are: Cl, Sp and Rs. The modification category includes: Fr, Ns and Vl.

Fr always modifies RD; Ns always modifies Cl. Consequently, the former articulation and latter manner need not be separately specified from the modification.

The voiced spirantal and closed consonants are considered unmarked as in Gleason's presentation. Both the unary feature specification advantages and the conventional analysis remain intact. There is loss of economy. Voiceless consonants require three features.

If the argument for the devoicing /h/ (which does not convince me) is assumed--no economy is sacrificed. Only two features are necessary to specify any of the twenty-three consonants under discussion. Further, if the single-feature-not-resonant argument (which also does not convince me) is accepted, a greater economy results. This economy is from the fusion of Rz and Do.

In the list below, certain consonants and features are enclosed in parentheses. These are consonants and features I would retain. They would likely be eliminated, however, from the Gleason perspective regarding voiceless consonants and single feature realizations.

/b/	<u>Lb</u> , <u>Cl</u>	/v/	<u>Lb</u> , <u>Sp</u>
/d/	<u>Ap</u> , <u>Cl</u>	/ð/	<u>Ap</u> , <u>Sp</u>
/g/	<u>RD</u> , <u>Cl</u>	/z/	<u>RD</u> , <u>Sp</u>
(/p/	<u>Lb</u> , <u>Cl</u> , <u>Vl</u>)	(/f/	<u>Lb</u> , <u>Sp</u> , <u>Vl</u>)
(/t/	<u>Ap</u> , <u>Cl</u> , <u>Vl</u>)	(/θ/	<u>Ap</u> , <u>Sp</u> , <u>Vl</u>)
(/k/	<u>RD</u> , <u>Cl</u> , <u>Vl</u>)	(/s/	<u>RD</u> , <u>Sp</u> , <u>Vl</u>)
/m/	<u>Lb</u> , <u>Ns</u>	/w/	<u>Lb</u> , (<u>Rs</u>)
/n/	<u>Ap</u> , <u>Ns</u>	/l/	<u>Ap</u> , (<u>Rs</u>)
/ŋ/	<u>RD</u> , <u>Ns</u>	/r/	<u>RD</u> , (<u>Rs</u>)
/j/	<u>Cl</u> , <u>Fr</u>	(/č/	<u>Fr</u> , <u>Cl</u> , <u>Vl</u>)
/ʒ/	<u>Sp</u> , <u>Fr</u>	(/š/	<u>Fr</u> , <u>Sp</u> , <u>Vl</u>)
/y/	<u>Rs</u> , <u>Fr</u>		

Unary feature specification may have implications for earlier English--particularly when voicing is considered the unmarked state. However, compare (Lamb, 1966) to (Lamb & Vanderslice, 1976) and (Reich, 1976). Old English "f", "s", and "sc" analyzed as /f/, /s/, and /š/ could be reanalyzed as /v/, /z/, and /ž/.

Figure Four

	<u>Lb</u>	<u>Ap</u>	<u>RD</u>	<u>Fr</u>
<u>Ns</u>	m	n	ŋ	
<u>Cl</u>	b	d	g	ʝ
<u>Sp</u>	v	ɖ	z	ʑ
<u>Rs</u>	w	l	r	y
<u>Cl</u>	p	t	k	č)
<u>Sp</u>	f	θ	s	š) <u>Vl</u>

Figure Five

	<u>Lb</u>	<u>Ap</u>	<u>RD</u>	<u>Fr</u>
<u>Ns</u>	m	n		
<u>Cl</u>	b	d	g	ʒ, y ²
<u>Sp</u>	v "f"	ɖ	z "s"	ʑ "sc"
<u>Rs</u>	w	l	r	y ¹
<u>Vl</u>	p	t	k	x

The investigators of the fissioning of the spirants focus on the introduction of voiced phones into initial and final positions. There is no quarrel with this. The process within native phonemes, however, could be viewed also as the spread of devoicing. I am not identifying a specific period of Old English. My list reflects that frequently found with some variation in introductory texts for Old English.

No essential differences in features are presumed. Vl effects only the stop or closed category which obviates the necessity to specify more than two features per consonant. I again intentionally exclude /h/ for reasons of scope. Fr is taken to have a slightly different effect on the stops. It modifies them more slit than grooved. The superscripts on the /y/'s indicate the earlier and later origins respectively.

The result is shown in figure five. I do caution that the Old English presentation is very speculative as I am not a trained historical linguist. Details could easily have been overlooked or oversimplified. In the list below, parentheses are used as in the previous discussion:

/b/	<u>Lb</u> , <u>C1</u>	(/p/	<u>Lb</u> , <u>V1</u>)
/d/	<u>Ap</u> , <u>C1</u>	(/t/	<u>Ap</u> , <u>V1</u>)
/g/	<u>RD</u> , <u>C1</u>	(/k/	<u>RD</u> , <u>V1</u>)
/v/ "f"	<u>Lb</u> , <u>Sp</u>	(/x/	<u>Fr</u> , <u>V1</u>)
/ð/	<u>Ap</u> , <u>Sp</u>	/ʒ, y ² /	<u>Fr</u> , <u>C1</u>
/z/ "s"	<u>RD</u> , <u>Sp</u>	/ž/ "sc"	<u>Fr</u> , <u>Sp</u>
/w/	<u>Lb</u> , (<u>Rs</u>)	/y ¹ /	<u>Fr</u> , (<u>Rs</u>)
/l/	<u>Ap</u> , (<u>Rs</u>)	/m/	<u>Lb</u> , <u>Ns</u>
/r/	<u>RD</u> , (<u>Rs</u>)	/n/	<u>Ap</u> , <u>Ns</u>

Conclusion

The role of the feature (phonon) in stratificational theory is well delineated. Its relevance to aspectual theory has been unnoticed. In published aspectual theory, attention has been directed primarily at defining and distinguishing: phone, phoneme and morphophone. Examples are: (Smith, 1967, 1968, 1970, 1972; Sustakoski, 1969; Hoffman, 1976; & Bergan, 1977.)

The necessity to record fine articulation features is essential for the aspectualist. It is especially true in variation studies whether geographical or social. The phonon, however, can be used as a guide for decisions of pattern maintainance or congruence. Ultimately, the phonon might be found to have a role of significance in the perception of contrast.

For aspectual theory, analysts are likely to determine phonons after their determination of the phonemic inventory by "Calvinist" procedures. In some cases, I suspect matrices formed by manner and articulation intersections will be slightly overdifferentiated. That is, two phonon-specified phones sharing one feature but differing in another may be potentially rather than actually contrastive. Likely candidates for such roles in various languages are: [n] and [ŋ].

Gleason's paper more than accomplished the task of starting my own thinking in new directions. I am appreciative of the opportunity to have heard and to have read it despite my reservations.

Note: Phonetic and phonemic representations are from (Trager, 1964) and (Trager and Smith, 1951).

Resources

Bergan, Hans R. 1976. "The Morphophone: A Reassessment of Phonology in (Di Pietro and Blansitt).

Di Pietro, Robert J.; and Blansitt, Edward L. Jr. (eds.) 1977. The Third LACUS Forum 1976. Columbia, South Carolina: Hornbeam Press, Inc.

Gleason, H.A. 1976. "Economy and Symmetry in English Consonants." in (Reich, 1976a).

Hoffman, Melvin J. 1976. "Problems from Implicit Premises in Classical Phonemics: Examination, Identification and Current Implications." in (Reich, 1976a).

Lamb, Sydney M. 1966. "Prolegomena to a Theory of Phonology." Language. 42: 536-573.

_____; and Vanderslice, Ralph. 1976. "On Thrashing Classical Phonemics." in (Reich, 1976a).

Reich, Peter A. (ed.) 1976. The Second LACUS Forum 1975. Columbia, South Carolina: Hornbeam Press, Inc.

_____. 1976. "Toward a Sequential Onset Feature Phonology of English." in (Reich, 1976a).

Smith, Henry Lee Jr. 1967. "The Concept of the Morphophone." Language. 43: 306-341.

_____. 1968. English Morphophonics: Implications for the Teaching of Literacy. Oneonta, New York: New York State English Council.

_____. 1970. "Linguistics: A Modern View of Language." in Harold Hungerford, Jay Robinson, and James Sledd (eds.) English Linguistics. Glenview, Illinois: Scott, Foresman and Co.

_____. 1972. "The Morphophone and English Dialects." in Lawrence M. Davis (ed.) Studies in Honor of Raven I. McDavid Jr. University of Alabama Press.

Sustakoski, Henry J. 1969. "Some Contributions of Linguistic Science to the Teaching of Reading." in James Walden (ed.) Oral Language and Reading. Champaign, Ill.: National Council of Teachers of English.

Trager, George L. 1958. "Paralanguage: A First Approximation." Studies in Linguistics. 13: 1-12.

_____. 1964. Phonetics: Glossary and Tables. Occasional Papers # 6. 2nd. Rev. Ed. Studies in Linguistics.

_____; and Smith, Henry Lee Jr. 1951. An Outline of English Structure. Occasional Papers # 3. Studies in Linguistics.

FROM PHONOSYMBOLISM TO MORPHOSYMBOLISM

Yakov Malkiel

University of California, Berkeley

There exists in our technical terminology a striking parallelism between such labels as apply to phonological and those relating to morphological phenomena. Thus, we make reference to phonemes and to morphemes; we speak of allophones as against allomorphs; we distinguish between phonotactics and morphotactics; we oppose phonology to morphology. (Interestingly enough, the -- once widely used -- term "semasiology" has, it would seem definitively, yielded ground either to "semantics" or to "semiotics", not least, perhaps, because it could not be so effectively paired off or contrasted with some other discipline, after its original counterpart, "onomasiology", failed to strike root.) Against this background of far-reaching symmetry one is tempted to ask why the term "phonosymbolism", acceptable to various (probably most) schools of linguistics and readily understood by tillers of neighboring fields and by many cultured laymen, apparently is not flanked by some such pendant as "morphosymbolism".

Judging by the conspiracy of silence displayed by the authors of technical linguistic vocabularies -- I mean Jules Maronzeau, Fernando Lázaro Carreter, Pei and Gaynor, and their peers -- the tag "morphosymbolism" has not, in fact, been experimented with by any responsible, respectable latter-day linguist. Perhaps it is unnecessary, simply because there exist no phenomena, at least not in the better-known natural languages, which can be so meaningfully identified. Alternatively it is arguable that such phenomena have indeed existed all along, but clandestinely, as it were; indisputably, the availability of an effective label is apt to bring into play or sharpen even the trained observer's latent alertness. Finally, it is conceivable and, actually, not unlikely that certain phenomena which will be here identified as manifestations of morphosymbolism have for years been familiar to practitioners of linguistics under some inconspicuous rival tag. If such turns out to be the case, one may legitimately ask himself whether the older designations or the name here advocated is more help-

ful, the yardstick being a combination of such desirable qualities as precision, explicitness, compatibility with other terms, and comprehensiveness.

Let us dwell for a few minutes on phonosymbolism, to gain a handy frame of reference. Phonosymbolism has a distinctly wider scope than onomatopoeia, which is narrowly suggestive of real-life sounds or noises; it also enjoys a somewhat better reputation among experts, presumably because its coinage is not traceable to hoary antiquity and, above all, because it has not figured prominently in amateurish or obsolete accounts of the presumable origin of speech.

An appeal to phonosymbolism simply means that the analyst endows the sound at issue with the ability to convey, in conjunction with other elements, a certain message of its own, with being, for once, the carrier of a minor, if not necessarily minimal, semantic content. This appeal presupposes the suspension of a very widely held, almost axiomatic assumption, namely that morphemes rather than phonemes are the smallest units of speech that are equipped with this power to transmit ingredients of meaning.

There is a considerable difference between relative and absolute sound symbolism. Supposing a sound is very rare in a given language, practically absent from its core vocabulary and confined to certain semi-acclimatized loan words or downright foreignisms: Under certain circumstances any of the few words containing that sound will, on the strength of that statistico-phonetic peculiarity, be immediately classed, even by naïve speakers, as referring to something outlandish, sophisticated, or *recherché*; as is true of French-inspired ô and û in English: petit beurre, bleu cheese, de luxe, provided of course we keep the pronunciation at this lofty, or, if you wish, pretentious plateau and avoid any plunge to the /peti bjur/ level. In many other languages in which ô and û are basic components of the native sound system, any Gallicisms containing these phonemes, trivial to the borrowers, would be that much poorer in expressivity or phonosymbolism. To revert to English, one could, I suppose, similarly analyze, perhaps with regard to a slightly older stage of the language, nasal vowels, as in some speakers' pronunciation of envelope, and /ž/, as in barrage, corsage, all of which, incidentally, makes genre the presumably most difficult lexical unit for native Anglophones.

So much for relative sound symbolism. Over against these examples one is tempted to place Otto Jespersen's celebrated attempt to show that the vowel

phoneme /i/ in practically all languages of the world has, or easily acquires, a diminutive-hypocoristic force. If Jespersen was right in this contention -- and a contemporary of his as discriminating as Edward Sapir, was willing to go along with him in this philosophy -- then this connotative quality of the unrounded high front vowel would approach the status of absolute sound symbolism, thus constituting a sort of language universal.

Somewhere between these two mutually exclusive poles, relative vs. absolute, it might be defensible to place those cases where certain phonemes in many, if not necessarily almost all, languages exhibit a symbolic force. Thus, /š/ and /č/ in hypocoristics, pet names, and other "intimate" words in a wide variety of by no means closely related languages -- cf. the series of affectionately colored proper names in Russian: Masha, Natasha, Sasha, Yasha -- Misha -- Aljoshka, Antoshka (beside Serjozha) -- Andrjusha, and the gamuts of "modifying" suffixes -accio, -iccio, -occio, -uccio of Italian (or, for that matter, -acho, -icho, -ucho of Spanish).

Though sound- or phonosymbolism is supposed to be a starting point rather than the principal topic on our agenda, I would like, at the risk of restating trivia, to set off three of its chief manifestations.

The first that comes to mind, is, obviously, onomatopoeia, with the speakers emulating nature in suggesting loud or characteristically spaced-out noises, some of which may be physiological, such as heavy coughs or convulsive laughter; witness Russ. xoxot and Sp. carcajada 'outburst of laughter', Gr. krächzen alongside Sp. graznar 'to caw, croak', Fr. cliquetis 'clanking, clatter, jingle', Sp. ronco 'hoarse, raucous'. We are dealing here with a rather piquant facet of "style in language".

The second manifestation is the artistic exploitation, by gifted and disciplined individuals, of just such potentialities. The segment of discourse on which the imaginative writer's virtuoso performance is brought to bear is normally no longer a single word, but a phrase, a sentence, or a whole paragraph. Through goal-directed selectivity, the poet, without minting any new words, strings those that his medium obligingly offers him in such fashion as to suggest certain quickly recognizable sound effects: the cascading roar of a waterfall, the rumbling of an approaching storm, the metallic clang of swords on shields, and the like.

Third, and most elusive (because we are rapidly approaching a border-line), even where any real accous-

tic resemblance is not, or no longer, directly recognizable — i.e., beyond the realm of legitimate onomatopoeia — we can still, cautiously, invoke sound symbolism where there prevails a clear-cut relation between a sound and a grammatical feature. Thus, in several Semitic languages the use of a back vowel in the first stem syllable or in the prefix of a verb form instantaneously suggests the passive voice. Take, among Hebrew intensive verbs, the contrast between pi'el and pu'al and, among causative verbs, the parallel contrast between hif'il and hof'al; or, to shift attention to Classical Arabic, compare fa'ala 'he did' with fu ila 'he was done', etc. Undoubtedly, the question as to why the Semites so intimately associated the back vowels with passivity is titillating, indeed tantalizing; but it is advisable to stop short of plunging into speculations. Whatever the underlying Weltanschauung, back vowels in certain well-defined positions have, for millennia, conjured up the image of passivity among typical speakers of Semitic.

To come closer to our goal and yank ourselves loose from the traditional ties to straight phonosymbolism, we shall now do well to examine one of the most inspiringly composed sections in Sapir's book Language. That string of essays, rather than a pedestrian textbook, even fifty-six years after its original publication date, remains, almost everyone agrees, a fountainhead of knowledge, wisdom, and felicity of verbalization. Chapter IV — the first of two consecutive chapters devoted to morphology — is titled "Form in Language: Grammatical Processes". After some stimulating introductory remarks, Sapir distinguishes between six fundamental categories of such processes, which he tags as "methods of conveying some sort of grammatical notion": first, juxtaposition of, at least, two words in a definite sequence; second, composition, i.e. "the uniting into a single word of two or more radical elements"; third, affixation, which, without hesitation, he views, on a world scale, as the incomparably most frequently employed of all grammatical processes; fourth, the internal vocalic or consonantal change, of the goose — geese or sing — sang — sung type, developed to its ultimate consequences in Semitic; fifth, reduplication, which for Sapir is the most "natural" device; and sixth, to use once more the author's own eloquent characterization, "the subtlest of all grammatical processes", namely "variations in accent whether of stress or pitch". It is the three excellent pages reserved, in Sapir's book, for the fifth class, i.e., for duplication, which I

am herewith inviting you to reexamine, after the initial encomium, in a necessarily critical vein.

From the outset Sapir admits that functional repetition may involve all or part of the radical element; in the latter eventuality, the label "reduplication" becomes slightly imprecise, since the arithmetic formula for the sum is not two, but something like one and a half, or one and two thirds. But this quantitative inequality is not all; even the basically repeated segment or -- more frequently -- the anticipatory slice may involve not so much straight iteration as subtle variation. Very appropriately, Sapir invokes the initial reduplication of certain older Indo-European languages used to form the perfect tense of numerous verbs, e.g., to cite his own illustrations, Sanskrit dadarsha 'I have seen', Greek leloipa 'I have left', Latin tetigi 'I have touched', and Gothic lelot 'I have left'. But this statement involves an obvious oversimplification. Straight loss alone of part of the consonantal skeleton, of the type for which Sapir prepares his readers, is indeed observable in Lat. mordeo 'I bite' vs. mo-mord-i 'I bit'. But most cases that come to mind turn out to be far more complex. In te-tig-i, for instance, it is not only the stem-final velar that is absent from the first while present in the second syllable, but the vowels have also been allowed to drift apart (ē vs. i); and once the present-tense, tang-ō 'I touch', is brought in, with its characteristic a and the no less conspicuous nasal element, it becomes clear that, on the level of Classical Latin, we are facing not plain reduplication, but richly orchestrated variation or modification. Cf. also canō 'I sing' and cecinī 'I sang'; pango 'I fix' and pepigi 'I fixed'; fallo 'I trip, deceive' and fecalli 'I tripped, deceived'; and many other such verbs.

However this is not all; a qualification in yet another direction of Sapir's optimistic arithmetic seems equally imperative. One is prepared to go along with him when he subsumes under his fifth category -- duplication -- both standard grammatical devices, of the type just outlined in connection with the Latin perfectives and ingredients of the familiar register of a language, such as E. goody-goody, to pooh-pooh, or, for that matter, sing-song, riff-raff, wishy-washy, harum-skarum, roly-poly (at this point, I am deliberately adducing Sapir's own illustrations, so as not to betray him). Again, as with the Latin verbs, it seems advisable to discriminate between plain repetition and variation. Moreover, by dint of searching

one discovers, precisely among the facetious, almost interjectional expressions -- especially those endowed with onomatopoeic overtones --, sequences of three rather than two elements; thus, a sudden shot or explosion can be evoked in German by bim-bam-bum and in Russian by pif-paf-puf. Granted the marginal status of these words in both languages, we have here before us examples of triplication rather than duplication. Let me therefore suggest that we use, in preference to reduplication, the broader label iteration; and that we clearly understand partial iteration as involving not only truncation, as in mord/eo, mo/mord/i, but also alteration. The single most suitable overall tag for all these overlapping, multidimensional processes would, in my estimate, be "elaboration on the root morpheme", in as much as "elaboration" implies expansion without narrowly restricting its quantitative or qualitative range.

If I may so far have given the unfortunate impression of quibbling with Sapir's terminology, there will be no further hair-splitting with respect to the scintillating formula under which that truly vibrant scholar bracketed those numerous mutually related functions which the evidence available to him -- picked from a generous assortment of familiar and, even more, exotic tongues -- prompted him to associate with this, let us noncommittally call it, "fifth" category of morphological processes. May I quote verbatim this composite Sapirian formula, if only to give you a chance to savor a small specimen of the author's bravura performance:

The process is generally employed, with self-evident symbolism, to indicate such concepts as distribution, plurality, repetition, customary activity, increase of size, added intensity, continuance.

Allow me to place extra-heavy emphasis, for transparent reasons, on the tell-tale phrase with self-evident symbolism. For, if I correctly interpret Sapir's own words and his thinking in this passage, his mention of symbolism in the context of a semiformal inventory of morphological processes or devices definitely implies a strong if unacknowledged belief in morphosymbolism. Though Sapir stopped short of coining or using this particular word, in large part because it was his general policy, in the context of that introductory book and of other essays as well, to avoid the use of grotesquely pedantic, polysyllabic terms, it follows very clearly from the entire passage that the author was a staunch believer in the agency of morphosymbolism.

More than half a century later, one is tempted to seize upon this illuminating remark and to ask himself whether any line -- preferably clear, if need be hazy -- can at all be drawn between phonosymbolism and morphosymbolism at their most sharply profiled.

Let us briefly revert to the reduplicative perfecta of Classical Latin and examine them in somewhat more leisurely fashion. They were already, so Indo-Europeanists report, on the verge of extinction at that late stage; Alfred Ernout, in his excellent *Morphologie historique du latin*, comes up with the lapidary statement: "Le parfait à redoublement est en latin un archaïsme en voie de disparition" (§280), and the Romance languages display only a very few relics, such as It. diedi 'I gave', stetti 'I stood'. But the ripe, not to say overripe, phase at which this grammatical pattern is observable in Latin carries with it certain advantages, of which not the least, for a quick overview such as ours, is the severely limited size of the corpus.

The majority of these none too numerous verbs, then, belongs to the -ere class, conventionally known as the third conjugation, and if we contrast the present tense (which pertains -- to use Antoine Meillet's phrasing -- to the "infectum"), with the perfect tense (as a sharply silhouetted representative of the "perfectum"), the opposition will produce the following effect, with special reference to the vowels and consonants both of the bare and of the extended root morpheme:

<u>cadō</u>	'I fall'	---	<u>cecidī</u>	'I fell'
<u>caedō</u>	'I strike'	---	<u>cecidī</u>	'I struck'
<u>currō</u>	'I run'	---	<u>cucurri</u>	'I ran'
<u>discō</u>	'I learn'	---	<u>didici</u>	'I learned'
<u>parcō</u>	'I spare'	---	<u>peperci</u>	'I spared'
<u>pariō</u>	'I bring forth'	---	<u>peperi</u>	'I brought forth'
<u>pēdō</u>	'I break wind'	---	<u>pepēdi</u>	'I broke wind'
<u>pellō</u>	'I drive'	---	<u>pepuli</u>	'I drove'
<u>pendō</u>	'I weigh, pay out'	---	<u>pependi</u>	'I weighed'
<u>poscō</u>	'I demand'	---	<u>poposci</u>	'I demanded'
<u>pungō</u>	'I prick'	---	<u>pupugi</u>	'I pricked'
<u>scindō</u>	'I often split, am splitting'	---	<u>scicidi</u>	'I once split'
<u>tendō</u>	'I stretch'	----	<u>tetendi</u>	'I stretched'
<u>tundo</u>	'I pound'	---	<u>tutudi</u>	'I pounded'

to which we are free to add cecini, fefelli, pepigi, and tetigi, already introduced at an earlier juncture, plus the unusually contoured verb memini 'I remember', which altogether lacks an "infectum". Upon isolating the characteristic vocalic sequences, we learn that we have run across such varied interplays -- eight all in all -- as (1) $\bar{e} \text{ -- } \bar{e}$ (fefelli, pependi, peperci, peperi, tetendi); (2) $\bar{e} \text{ -- } \bar{e}$ (pependi); (3) $\bar{e} \text{ -- } \bar{i}$ (cecidi, cecini, memini, pepigi, tetigi); (4) $\bar{e} \text{ -- } \bar{i}$ (cecidi); (5) $\bar{e} \text{ -- } \bar{u}$ (pepuli); (6) $\bar{i} \text{ -- } \bar{i}$ (didici, scicidi); (7) $\bar{o} \text{ -- } \bar{o}$ (novosci); and (8) $\bar{u} \text{ -- } \bar{u}$ (cucurri, pupugi, tutudi). If we decide to throw in, for good measure, the few relevant verbs which another conjugation class, namely that of the -ere verbs, affords us, no radically different picture is apt to emerge. To momordi 'I bit', already introduced as evidence, we are now free to add pendeo 'I hang', which shares with pendo the past pependi; spondeo 'I pledge' beside spondo; tondeo 'I clip, shear' alongside tondo; and, if you wish, the archaic couple teneo 'I hold tight, keep' flanked by tetini, later replaced by tenui: we still arrive at such gamuts as $\bar{e} \text{ -- } \bar{e}$, $\bar{e} \text{ -- } \bar{i}$, and, with discernibly increased frequency, $\bar{o} \text{ -- } \bar{o}$. There can, of course, be no talk of comparable gamuts in the case of the ultrabrief, maverick verbs for 'to give' and 'to stand'; but dō -- dedi and stō -- steti coincide in confirming the frequency of $\bar{e}$ without tipping the scales in its favor as a predictable marker.

We are faced, then, with a wide variety of vowels (only few are absent, notably a) and of sequences of vowels as well; this state of affairs, fairly long ago, led Ernout to remark: "Le parfait à redoublement latin n'a donc pas de vocalisme propre" (Morphologie. . ., §271). As for the consonants involved, one grants a striking recurrence of occlusives, both voiced and voiceless, but the present-tense forms also show a profusion of other types of consonants, e.g. n and r and l and s and f, some of which, but by no means all, disappear in the perfect tense; witness the eloquent case of fefelli as an example of perseverance.

If we assess the exiguous Classical Latin corpus in its own terms and for its own sake rather than hastening to subordinate it to any comparative and reconstructive designs, we can, I submit, isolate a fairly pure brand of morphosymbolism and see it at work. For, assuming it is true that the surrounding consonants here play a very limited part and that the sets of nuclear vowels basically involve the echoing of the root vowel, better still, of any root vowel whether it be a front or a back vowel, with a modicum of adjustments to phono-

logical 'laws' or 'rules', as in the shifts of stressed a to unstressed e or i (tangō -- tetigī), then it is incontrovertibly partial reduplication alone and viewed in abstracto that constitutes the irreducible nucleus of the entire process. To put it differently, we are privileged to watch a symbolic process, as Sapir rightly sensed, set in motion to obtain a neatly circumscribed morphological end, namely the derivation of a past tense from an aspectually contrastable present tense, and virtually demuded of any phonic constraints. It may be difficult for us moderns to grasp the underlying symbolism -- it certainly seems psychologically easier to apprehend the use of reduplication for the extraction of a plural from a singular, or of a mass-noun from a one-of-its-kind noun, or of an augmentative from the label for a standard-sized object. By stretching or straining our imagination, we could speculate that within the frame of certain archaic or exotic cultures the present figured as a dot or a short stretch and the past as an accumulation of such stretches, with a hint of plurality; but this is sheer speculation, of the same order as the ultimate reason for the use of u in Semitic passives, and those who may want to test it will doubtless keep in mind that the perfect tense of Latin initially stressed a reference not so much to a past along the time axis as to a completed state or action.

More realistic than such futile gropings is a quick glance, which we suspended a few minutes ago, at archaic, pre-classical Latin. At that stage there was a sweeping tendency to mark the perfect tense by a short e, one that often matched an epsilon in Classical Greek. In the oldest texts one need not be surprised to find traces of cecurrī 'I ran', memordī 'I bit', peposcī 'I demanded', pepugī 'I pricked', spondeī 'I vowed'. This is neither the place nor the hour to discuss what inferences can be drawn from such findings for projection onto the Proto-Indo-European foundations: the Greek evidence -- mēmōna beside Lat. meminī, dēdomai beside Lat. dedī, hēsteka beside Lat. stetī -- favors, I repeat, the positing of e as an ancient marker of the "perfectum" to an overwhelming degree, though probably not on an absolute scale, since, e.g., the vocalism of tutudī 'I founded', we are told by experts, receives collateral support from such Sanskrit forms as tutōḍa or, in the middle voice, tutudé. In any event, there was a strong tendential association of a specific vowel, in addition to reduplication, with a whole wing in the

edifice of the verbal paradigm; to this extent it is indeed arguable that in pre-classical Latin the process involved a less pure or less exclusive variety of morphosymbolism than at the more familiar stage previously sketched out.

Having made a case, on theoretical grounds, for form symbolism as against sound symbolism, and having inspected in some detail, at least, one almost unadulterated sample of the former, we are now led to raise the classic question that comes to mind whenever such distinctions are established: Are we confronted by two neatly delimited, discrete forces, or can one expect to discover a gradual transition between them? I happen to lean toward the second hypothesis.

Adequate examples of phonosymbolism in the raw are the two cross-linguistic studies by Jespersen, one, already alluded to at the start, dealing with the suggestion of smallness by means of the vowel i; the other, slightly earlier, and available solely in Danish garb, centering about the evocation of roundness through the bilabial nasal m. Taking his cue from a few stimulating remarks by Georg von der Gabelentz and adding to them some random observations of the uninhibited speech of small children, Jespersen, first of all, moored his ideas to certain anatomical and physiological givens, then examined -- e.g., in his study of the symbolic value of the vowel i -- words for 'little', those for 'child, tot, baby', or 'young [= small] animal', those for small-sized objects, the configuration of diminutive-hypocoristic suffixes, etc., pointing out the spectacular recurrence of /i/ in all these contexts. Morphological considerations are here reduced to the barest minimum; thus, the division into root morphemes and affixes is carried out exclusively for the classificatory convenience of the reader. And the assortment of the languages drawn upon is motley -- genetically, typologically, geographically, and temporally. One cannot possibly move closer to the realm of language universals and be at a farther remove from the structures, either phonic or grammatical, of individual languages. So, if we accept, albeit not uncritically, Jespersen's central thread of thought, we have here before us somewhat hastily assembled and almost grossly assorted instances of unalloyed phonosymbolism, with Engl. little, imp, kid, chit, pygmy, bit, jiffy, slit, chink, Sp. niño, chico, animalito, asnillo, pequeñino, and many German and Scandinavian words, plus a few culled from other languages, including Hungarian, forming a sort of crazy quilt. At the other extreme of the gradient one is tempted to place a case like that of

the reduplicative perfects in Classical Latin, where the physical substance of the vowels and consonants almost counts for nothing and all that matters is the partial iteration of the characteristic root morpheme, serving to convey a conspicuously neat grammatical message. For the intermediate, transitional section of the gradient we could then defensibly reserve such contexts where any onomatopoeic suggestion through phonic substance seems to have completely evaporated, but nevertheless certain carefully chosen phonemes or configurations of phonemes perform one rather tidily defined service.

With this aim in mind, let us cursorily examine the semijocose modern English reduplicatives highly characteristic of the informal discourse and of the kindergarten milieu. Two major pioneers, Jespersen and Sapir (who, incidentally, for years were engaged in lively correspondence on -- *inter alia* -- precisely this matter) were agreed on the wisdom of classing playful formations such as ding-dong and hodge-podge with the more serious and more thoroughly-standardized reduplicative devices found in a great many languages of the world; not for nothing, a favorite student of Sapir's, namely Mary R. Haas, in 1942, compiled an excellent preliminary inventory of, and engaged in an inquiry in, such formations in contemporary American English, a survey which can be expanded and further refined along certain lines.

We must, first of all, discriminate between vocalic apophony and consonantal apophony; contrast riff-raff with hurly-burly. Each category can be further subdivided, the former principally according to its syllabic structure and its vocalic kernel(s), the latter chiefly on the basis of syllable-initial consonants. The most characteristic variety, in the ranks of vocalic apophony, is the type involving two monosyllabic components, very often i -- a: chit-chat, fiddle-faddle, flim-flam, (k)nick-(k)nack, mish-mash, riff-raff, spish-splash, tick-tack, whim-wham, zig-zag. Three rarer patterns of interplays between vowels have been identified: first, criss-cross, ding-dong, sing-song; second, slip-slop, tick-tock, plus dial. mish-mosh; and, third, see-saw. Then there exist disyllabic elaborations on these basic schemes, chiefly executed with the aid of the all-purpose suffix or suffixoid -y: dilly-dally, shilly-shally, ticky-tacky, whims(e)y-whams(e)y, wishy-washy (though one hears increasingly /waʃi/). Distinctly rarer is the suffixoid -er, as in pitter-patter, and teeter-totter, and -um, which may have its ultimate roots in mock-learned tradition: crinkum-

crankum.

The contingent of words representing consonantal apophony exhibits an entirely different structure. Here one encounters a handful of rather isolated formations, which leave the analyst wondering whether they should be bracketed by the dominant vowel (in which case namby-pamby, raggle-taggle, and razzle-dazzle belong together, fuddy-duddy flanks munbo-jumbo, and roly-poly stands alone) or by the syllable-initial consonant, in which eventuality roly-poly can be allowed to join raggle-taggle and razzle-dazzle since in all three the first syllable begins with /r/ and the second with a voiced or voiceless stop. And this configuration is far from constituting the only alternative. More significant, one discovers before long two larger and highly productive groups, particularly dear to children: first, lexical items and nicknames whose second component, obligatorily, begins with a w-: monosyllabic big-wig and disyllabic Charlie Warlie, ducky-wucky, flimsey-wimsey, fuzzy-wuzzy, palsy-walsy, piggie-wiggie, with the already familiar protean suffixoid -y, spelled in three different ways, virtually reigning supreme; and, second, such -- even more numerous -- formations as involve a word-initial /h/. These can be more finely subdivided on the strength of syllabic architecture, with a minority of monosyllables (including hobnob, hodge-podge, hubbub, hubble-bubble, humdrum) facing a majority of disyllables differentiated by characteristic suffixoids, e.g. hanky-panky, hoity-toity, humpty-dumpty, hurly-burly; helter-skelter, mugger-mugger; harum-scarum alongside hocus-pocus. In all these instances still subtler subdivision on the basis of the vowels and consonants most prominently placed would of course be entirely legitimate. A combination of mono- and di-syllabic nuclei is exceedingly rare; only the slangy synonyms honky-tonk and ricky-tick, both designating cheap, noisy cabarets, have come to my attention, and their erratic, asymmetric structure conceivably serves to evoke a tinkling sound or a kind of syncopated music.

These bubbly, fluffy, ephemeral words, of which I have offered only a limited selection, pose a number of challenging problems, not a few of them genetic. Thus, mischmasch, ticktack, and zickzack occur also in German (and the second and the third have counterparts in other languages as well); so there may be an admixture of migratory formations, Wanderwörter, which unavoidably provoke special complications, much as ping-pong, involving a trite echoic reduplication, at one point in history had the advantage of, at least, soun-

ding and looking authentically Chinese. One discerns other genetic problems that circumstances force us to skip: pell-mell is demonstrably French, as is presumably also riff-raff, both of which call to mind a somewhat differently architected bric-à-brac, also imported from across the Channel, and a wealth of such native binomials of similar but not identical appearance as spick and span, with a connective (and = 'n) and a modicum of variation in the syllable-final consonants. Finally, there are in store for the language historian all sorts of etymological puzzles and surprises, as when shally turns out to be traceable to the question Shall I? hinting at the perennial skeptic or "cunctator", while criss-cross, literally, once meant 'Christ's cross'. In general, the second ingredient emerges as the typical nucleus of the whole (except, naturally, for the contingent of w- words, such as fuzzy-wuzzy; witness chit-chat, razzle-dazzle, splish-splash, but there are exceptions, including whims(e)y-whams(e)ys, where whims(e)y outranks its partner, and occasionally the hierarchy is left indeterminate, as with sing-song.

The central question before us is whether the hard core of this multi-faceted derivational scheme is phonic or morphic? My own feeling is that it is, or has in course of time become, predominantly morphic. There may be an element of pure onomatopoeia in the musically slanted formations ticktack 'a recurrent sound like the ticking of a clock', sing-song 'unvarying rise and fall of tone', ding-dong 'the sound of a bell struck repeatedly', plus, I repeat, in the effectively bizarre, almost exaggeratedly realistic honky-tonk and ricky-tick. Let me further grant that syllable-initial fl- (as in flim-flam 'nonsense, rubbish', flimsey-wimsey 'ill-conceived, inadequate'), sl- (as in slip-slop 'shallow, pointless talk', which somehow echoes slipshod), and the slightly better endowed spl- (as in splish-splash 'mass of splashed water, mud') are all three expressive, or vaguely suggestive, or at least uproariously amusing in numerous lexical contexts of modern English, and let me emphasize my awareness of the reappearance, syllable-finally, of equally stimulating dyadic consonant clusters equipped with l as their second component, witness fiddle-faddle 'nonsense, trifles', hubble-bubble 'bubbling sound, uproar', raggle-taggle 'odd mixture, crazy quilt, motley' (in elaboration on less ostentatious ragtag), as well as razzle-dazzle 'flashy display'. Finally, the frequency of the word-medial se-

quence nasal + homorganic consonant is hardly fortuitous; cf. namby-pamby 'insipidly pretty' (originally the satirical name of the 18th-century English poet Ambrose Philips), mumbo-jumbo 'gibberish' (initially the designation of an idol among certain West African tribes), hanky-panky 'trickery, sleight of hand', Humpty-Dumpty 'a short squat character in an old English nursery rhyme, a personification of an egg'. Nevertheless, on balance, in the majority of cases the choice of the particular vowels and consonants, with due allowance for their undeniable acoustic properties and the semantic implications of these properties, seems less significant than the mere fact of the repetition, with certain well-defined and widely understood variations, of a qualifying root morpheme, made in an effort to convey a certain message, predominantly one of 'disorder, confusion', or 'rubbish, trash', fig. 'rabble'. This recurrent central image emerges from the suggested erratic, whimsical succession of moves or steps; contrast a zig-zag line, recognizable by its crazy ups and downs, with a straight line, evocative of orderliness and purposefulness. The distorted echoing of sounds involves, if you wish, by its sheer configuration far more than by specific physical qualities, a modicum of synesthesia. Even where the semantic development has advanced very far, something of the original hint of 'chaos' or 'funny assortment' shines through. Thus, a residual affinity of hotchpotch ('thick stew (of various meats and vegetables)') and of its more familiar and less concrete doublet hodgepodge 'jumbled mixture, mess, medley' -- or, for that matter, of helter-skelter 'hurried and confused' -- to chitchat 'light, informal talk' is immediately recognizable, even though the prevalent focus has discernibly shifted from 'aimlessness', 'loss of any sense of orientation' to deliberately and even artistically cultivated 'frothiness'.

To round out the picture, we may well cast a quick glance at one more multi-dimensional situation. In modern German, certain classes of, chiefly, paroxytonic words, marked by a characteristic nuclear vowel and an equally well-defined ending, namely -e or -er, almost advertise their meanings within the confines of a pre-determined grammatical status. Which consonant (or consonant cluster) occupies the word-initial and the word-medial slots seems not to matter; neither is the picture affected by the addition of a prefix. To be more explicit, if the nuclear vowel is either ö or ü, or else, in very formal discourse, g, one detects a limited number of recurrent semantico-grammatical patterns.

(A) Assuming the word under scrutiny is a verb, the chances are that the form at issue is a past subjunctive

(to be specific, the 1st and the 3rd person sg.), thus:

(a) böge (from biegen 'to bend'), böte (from bieten 'to offer'), flösse (from fließen 'to flow'), fröre (from frieren 'to freeze'), ge-nösse (from ge-nießen 'to enjoy'), kröche (from kriechen 'to creep'), löge (from lügen 'to tell a lie'), schlösse (from schließen 'to close, lock'), schösse (from schießen 'to shoot'), schöbe (from schieben 'to shove'), zöge (from ziehen 'to draw');

(b) stünde (from stehen 'to stand'), stürbe (from sterben 'to die'), würde (from werden 'to become'), wüste (from wissen 'to know');

(c) bräche (from brechen 'to break'), brächte (from bringen 'to bring'), hätte (from haben 'to have'), käme (from kommen 'to come'), läge (from liegen 'to lie [out-stretched]'), sänge (from singen 'to sing'), täte (from tun 'to do'), wäre (from sein 'to be'), zwänge (from zwingen 'to force').

Where the infinitive and the entire paradigm of the pres. ind. already have a nuclear ö, ü, or ä, the corresponding form with the characteristic ending -e will be interpreted as a pres. subj. -- which, in actual use, happens to be very close to the past subj. Thus we arrive at:

(a) ent-blöße (from ent-blößen 'to bare'), höre (from hören 'to hear'), löse (from lösen 'to loosen, solve'), möge (from mögen 'to like'), schwöre (from schwören 'to swear'), ströme (from strömen 'to flow'), töne (from tönen 'to sound'), töte (from töten 'to kill');

(b) füge (from fügen 'to attach'), küsse (from küssen 'to kiss'), schüre (from schüren 'to fan');

(c) ätze (from ätzen 'to corrode'), krähe (from krähen 'to crow').

All these forms also do service as singular imperatives.

Note that dränge is, concurrently, past subj. of dringen 'to enter by force' and pres. subj. of drängen 'to urge'.

(B) Assuming the word under investigation is an adjective, then each of the metaphonized root vowels ö, ü, ä, accompanied this time by the fuller ending -er, will very often identify the form at issue as a comparative degree in its basic design, thus:

(a) größer (from groß 'big, great'), höher (from hoch 'high, tall'), röter (from rot 'red');

(b) dümmer (from dumm 'stupid'), krümmer (from krumm 'crooked');

(c) kränker (from krank 'sick'), schärfer (from scharf 'sharp'), schwächer (from schwach 'weak'), schwärzer (from schwarz 'black'), stärker (from stark 'strong, stark, sturdy').

In this category, there are numerous exceptions in

both directions, with positive and comparative relating, counter to expectation; as in toll 'rabid' -- toller, dumpf 'dumb' -- dumfer, schlank 'slim' -- schlanker, schön 'beautiful' -- schöner, kühn 'bold' -- kühner, schräg 'slanted' -- schräger; nevertheless, the dominant pattern is unmistakable.

(C) Finally, where the word under our microscope lens turns out to be a noun, one recognizes two rival possibilities.

Assuming the word is a derivative and the ending is -e, there is an excellent chance that we are facing an adjectival abstract, which in a few instances may have been secondarily concretized:

(a) größe 'size, greatness' (groß), höhe 'height' (hoch), höhle 'cave', orig. 'cavity' (hohl), röte 'redness, red spot' (rot);

(b) fülle 'abundance' (voll), kurze 'brevity' (kurz);

(c) blässe 'pallor' (bläß), nahe 'proximity' (nah), nässe 'humidity' (naß), schwäche 'weakness' (schwach), schwärze 'blackness, black substance' (schwarz).

An exception would be süß 'sweet' beside süße 'sweetness'; characteristically, the latter is yielding pride of place to süßigkeit, especially in reference to 'sweets'.

The alternative is for nuclear ö, ü, or ä to signal a plural, in conjunction with the endings -e or, more sparingly, -er, thus:

(a) chöre 'choirs' (from chor), köpfe 'heads' (from kopf), schlösser 'castles' (from schloß), stöße 'hits' (from stoß), ströme 'streams' (from strom);

(b) grüße 'greetings' (from gruß), stühle 'chairs' (from stuhl), stürze 'downfalls' (from sturz), türme 'towers' (from turm);

(c) schwänke 'farces' (from schwank).

Here the margin of exceptions is distressingly wide: flöte is a single 'flute'; kröte refers to just one 'toad', mühe means 'hard work', and krahe stands for an isolated crow. But the pattern has not been completely overlaid.

Time permitting, one could similarly examine, testing them for their predictability, diminutives (krume 'crumb' -- krümchen), adjectival verbs (schwächen 'to weaken'), attenuating adjectives (kränzlich 'sickly'), and verbal abstracts, particularly those announced by the prefix ge-: getöse 'tumultuous noise, din', from tosen 'to roar', gewächs 'plant', lit. 'plant growth', from wachsen 'to grow'.

In none of the instances discussed or merely hinted at can one claim that ü, ö, or ä per se carries any specific message, comparable to what Jespersen and Sapir thought of i or m, cross-culturally. Here we are facing not only a language-specific state of affairs, but also

one tightly controlled by the interplay of a severely limited number of form-classes. Selected sounds, under narrowly defined conditions, to be sure, may carry a probabilistic grammatical message in German -- but the key to it is the preconditioned listener's familiarity with the morphological edifice of the given language.

In sum: The domains of phonosymbolism and morphosymbolism, as here visualized, not only abut on each other, but overlap and interlock. One can invoke phonosymbolism, pure and unalloyed, in a relatively small number of situations. The classic example remains, of course, onomatopoeia: the best possible imitation, in a given medium, of a thunderclap, of the roaring of wild animals, of the clatter of hoofs on stony ground. One is further free to place within straight phonosymbolism the partial suggestion of meaning through the position of speech organs, e.g., through a characteristic aperture of the mouth or a conspicuous rounding of the lips. Remember the attempts made by scholars as level-headed as Jespersen and Sapir to attach a miniaturizing affect to the vowel i, an aggrandizing effect to the vowel a, etc., across border-lines dividing individual languages and whole language families. By straining one's imagination and stretching one's definitions one can conceivably go one step further and agree to call phonosymbolic certain culture-specific as well as sound-system-specific associations between sounds, or combinations of sounds, and messages conveyed, provided the concrete roles of specific sounds can be explicitly stated, as when the vowel u, in Classical Arabic and its congeners, suggests passivity, the consonant ch, in Spanish, announces something "cute", exciting, or intriguing, word-initial g-plus consonant, in English, is associable with diminution (small, slight, slender, slim). One way or the other, the physiological events of sound production and sound perception are implicated in phonosymbolism, however diluted the relation between the three features of sound, meaning, and real-life happenings may have become with the passage of time.

At the opposite end of the scale, the residual reduplicative "perfecta" of Classical Latin, if not of Proto-Latin, furnished a telling example of morphosymbolism almost unadulterated. The concrete sounds exerted scant influence on the course of events, except that A happened to be absent. Although the corpus was exiguous, we discovered as many as eight possible successions of vowels. All that mattered to that speech community was, it appears, the elaboration on the root morpheme, usually in the form of partial reduplication. The formula, cut to the bones, ran thus: Juxtaposed shorter form plus fuller form of a verb morpheme equals a certain past tense, aspectually colored.

In several contexts, phonosymbolism and morphosymbolism work in unison. One discerns certain favorite vowel and consonant sequences and certain preferred syllabic configurations in the semijocose formations chit-chat, dilly-dally, mumbo-jumbo, palsy-walsy, fuzzy-wuzzy with which colloquial English regales its speakers. The German vowels ö and ü, less so at present ä, often convey messages and arouse expectations, but just what they allow the speakers to encode will hinge almost entirely on the form-class involved, so that the listener's morphological consciousness has to filter the message entrusted to these three metaphonized vowels. Once again, we are at an intersection of phono- and morpho-symbolism.

It seems to me that by balancing, in our analyses, the effects of phonosymbolism against those of morphosymbolism, we stand a good chance of ploughing over a fertile field, which has almost been left fallow for over thirty years, after the disappearance from the scene of two inspired pioneers: Jespersen and Sapir.

FOOTNOTE

¹ Jespersen's and Sapir's separate but gradually converging interests in the problems here discussed reached their climax in the decade 1918-28. The two friends not only exchanged letters on this subject, but Sapir, in the early or mid 'twenties, apparently sent his correspondent a large collection of raw data, perhaps because he despaired of being able to put it to use himself. Later, however, he reverted to the subject, this time testing the divergent reactions of average speakers through nonsense words containing the vowels A and I (1929). It might be worthwhile to salvage and publish this trans-Atlantic exchange of letters and to add to this portfolio the relevant papers, or sections of books, translating in the process certain trail-blazing pieces by Jespersen from Danish into English. Spitzer's editorial comments (1929) list certain critical reactions and parallel explorations.

REFERENCES

- Ernout, Alfred. 1953. Morphologie historique du latin. Rev. 3rd ed. Paris: C. Klincksieck.
 Haas, Mary R. 1942. "Types of reduplication in Thai (with some comparisons and contrasts taken from English)", Studies in Linguistics 1.4.1-6.

- Jespersen, Otto. 1918. "Nogle Ken-Ord". Studier tilledede Esaias Tegnér den 13 januari 1918, 49-55. Lund: C.W.K. Gleerups Forlag.
- 1922a. "Symbolic value of the vowel I", Philologica (London & Prague) 1.1-19. Reprinted, in 1929, in Leo Spitzer (ed.), Meisterwerke der romanischen Sprachwissenschaft 1. 53-68, München, Max Hueber; and in [1962], with a few alterations and many additions, in O.J., Selected Writings, London: George Allen & Unwin Ltd., and Tokyo: Senjo Publishing Co., Ltd., 557-577.
- 1922b. "Lydsymbolik", Nordisk Tidskrift för Vetenskap, Konst och Industri (Stockholm).
- 1925. Mankind, Nation and Individual from a Linguistic Point of View. Oslo: H. Aschenhoug & Co. (W. Nygaard). Reprinted, 1946, 1964, Bloomington: Indiana University Press.
- Lázaro Carreter, Fernando. 1968. Diccionario de términos filológicos. Rev. 3d ed. Madrid: Gredos, S.A.
- Marouzeau, J. 1943. Lexique de la terminologie linguistique: français, allemand, anglais. Rev. 2d ed. Paris: Paul Geuthner.
- Meillet, Antoine. 1931. Esquisse d'une histoire de la langue latine. Rev. 2d ed. Paris: Hachette.
- Pei, Mario A., & Frank Gaynor. 1954. A dictionary of linguistics. New York: Philosophical Library.
- Sapir, Edward. 1921. Language; an introduction to the study of speech. New York: Harcourt, Brace, & Co.
- 1929. "A study in phonetic symbolism", Journal of Experimental Psychology 12.225-39.
- Spitzer, Leo. 1929. [Editorial comments on Otto Jespersen, "Symbolic value of the vowel I", in Meisterwerke 1, 355-6.]

LA CHUTE DU /l/ DANS LES ARTICLES ET LES PRONOMS CLITIQUES EN FRANÇAIS QUÉBÉCOIS

Laurent Santerre, Danielle Noiseux et Luc Ostiguy.

Université de Montréal

L'objet de notre étude porte sur la chute du /l/ en français québécois. Bien que le sujet ait déjà été traité, nous jugeons malgré tout que notre étude conserve sa pertinence. En effet, Pupier & Légaré (1972) proposaient une analyse phonologique, laquelle nous semble plus ou moins satisfaisante. A notre avis, une analyse morphologique explique d'une façon plus adéquate le phénomène de l'élision du /l/. Nos données ont été extraites du corpus Sankoff-Cedergren et à partir de l'intuition de locuteurs non montréalais.

A cela s'ajoute une étude socio-linguistique. Contrairement à l'étude Sankoff-Cedergren (1971), qui ne portait que sur 16 individus, la nôtre porte sur 100 individus, couvrant ainsi presque la totalité du corpus. A cette fin, nous avons retenu 10 minutes d'enregistrement pour chaque locuteur.

Les articles: le, la, les l'

Le contexte favorable à la chute du /l/ est la position intervocalique. Ainsi le /l/ disparaît entre deux voyelles:

- (1a) "dans les maisons" [dɛmɛzɔ̃]
- (1b) "bâtissaient la maison" [batɪsɛamɛzɔ̃]
- (1c) "sonner la cloche" [sɔnɛaklɔ̃]
- (1d) "je jouais à la corde" [ʒwɛakɔ̃rd]
- (1e) "j'ai la grippe" [ʒaɛʁɪp]

Il est cependant maintenu quand il est précédé d'une consonne:

- (2a) "je regarde la télévision" [ʒɛʁɡɑrdlatɛlɛvɪzjɔ̃]
- (2b) "pour pas que la cuve coule" [pɔʁpɔklakɪvku]
- (2c) "vers les alentours" [vɛʁlɛzalɛ̃tɔ̃ʁ]
- (2d) "avec la maîtresse" [ɛklamɛtʁɛs]

Pour ce qui est de "le", le /l/ ne peut tomber, parce que la règle d'effacement du /ə/ s'étant appliquée avant celle du /l/, il se retrouve suivi d'une consonne:

- (3a) "puis le bas" [pɪlbɔ̃]
- (3b) "dans le coin" [dɑ̃lkɔ̃]
- (3c) "c'est bien le fun" [sɛbɛlfɔ̃]
- (3d) "c'est le contraire" [sɛlkɔ̃tʁɛ]

La règle du chute du /l/ pour les articles pourrait donc se lire comme suit:

$$(I) \quad l \rightarrow \langle \emptyset \rangle / [\text{syll.}] - [\text{syll.}] \quad \left. \vphantom{l \rightarrow \langle \emptyset \rangle} \right\} \text{article}$$

Il existe cependant un contre-exemple qui s'applique à l'article féminin "la" quand il est précédé de "de". Nous retrouvons soit la forme:

(4a) "manger de la misère" [mãʒedlamizɛr]

soit la forme:

(4b) "manger de la misère" [mãʒedamizɛr]

La chute du /ə/ s'étant appliquée, le /l/ se retrouve précédé d'une consonne et ne devrait pas pouvoir tomber. Or dans (4b), il tombe. Nous croyons que ceci est dû à une application tardive de la contraction qui a donné: "du" pour "de le"
"des" pour "de les"

et qui donne aujourd'hui la forme: [da] pour "de la".

En ce qui concerne l'article élidé "l' ", règle générale, il ne tombe pas même s'il est à l'intervocalique.

(5a) "à l'usine" [alyzɪn]

(5b) "dans l'économie" [dãlekɔnɔmi]

Ce phénomène est dû à la présence d'une frontière de morphème qui suit immédiatement le "l' ". On modifiera donc la règle (I) de cette façon:

(2) $l \rightarrow \langle \emptyset \rangle / [\text{syll.}] - [\text{syll.}] + \text{article}$

L'article élidé peut toutefois tomber dans les expressions courantes du type: "préposition + l' + nom".

(6a) "à l'école" [e:kɔl]

(6b) "à l'hôpital" [a:pital]

(6c) "dans l'après-midi" [dã premidi]

(6d) "c'est de l'ouvrage" [seduvraʒ]

Nous supposons que dans ces cas-ci, la chute du /l/ est due à la grande fréquence d'emploi qui aurait annihilé la frontière de morphème. Ainsi les locuteurs feraient une réanalyse sans frontière dans des expressions courantes.

Les pronoms clitiques

Nous voulons immédiatement faire une mise au point concernant les pronoms clitiques en français québécois. Ce que nous entendons par pronom clitique, ce sont les pronoms qui sont non-accentuables en structure sous-jacente.

Prenons l'exemple "donnes-moi la". Ici, il n'y a pas de problème, car l'accent est sur le pronom. Mais si on prend l'exemple "donnes-moi la pas", l'accent tombe sur le "pas" et non sur le pronom. Nous considérons cependant que le pronom est accentuable en structure profonde, et par le fait même, non-clitique. Ainsi, pour nous, la seule position favorable à l'apparition de ce que nous appelons les clitiques, est la position pré-verbale. Autrement dit, les clitiques sont toujours pré-verbaux.

Le /l/ de "les" et "la" tombera donc facultativement, alors que celui de "le" restera à cause de la chute du /ə/.

Le pronom élidé "l' " restera aussi, mais cette fois, à cause de la frontière de morphème. Pour ce qui est du pronom "lui" nous ne nous en occuperons pas, parce que nous croyons que ce n'est pas la forme /lɥt/ mais /ɥ/ qui en est la forme sous-jacente. Quant à "leur", nous y reviendrons un peu plus loin.

A première vue, nous serions tentés de croire que, tout comme pour les articles, le contexte favorable à l'effacement du /l/ est la position intervocalique.

- (7a) "on les regardait" [ʃergarde]
- (7b) "ça les intéresse pas" [sæzêterespo]
- (7c) "qui va la réparer" [kɪvɔarepare]
- (7d) "je vais aller la reconduire" [ʒvɔalearkõdqɪr]

Par contre, nous remarquons que le /l/ peut tomber, même précédé d'une consonne.

- (8a) "elle me les paye" [amɛpɛj]
- (8b) "je la veux" [ʒavø]
- (8c) "je les veux" [ʒevø]

Ici, le /ø/ tombe suivant sa règle de chute et le /l/ se retrouve précédé d'une consonne, ce qui ne l'empêche pas de disparaître.

Il existe aussi beaucoup d'autres cas où le /l/ peut disparaître quand il est précédé d'une consonne:

- (9a) "Marc la veut" [markavø]
- (9b) "Guylaine les prend pas" [ɛi!ɛneprãpo]
- (9c) "ils peuvent les chercher" [ipɛvɛʃɛrʃɛ]

Ainsi le /l/ des pronoms tombe indépendamment du type de phonème qui le précède. Ceci nous amène donc, non pas à modifier la règle (2), mais bien à en postuler une nouvelle pour les pronoms:

- (3) $l \longrightarrow \langle \emptyset \rangle / - [\text{syll.}]$ } clitique

Il nous faut cependant modifier quelque peu cette règle pour nous permettre d'éliminer le pronom "leur" qui ne perd jamais son /l/. Toutefois, dans certaines régions, le /l/ peut se palataliser:

- (10a) "il leur donne" [i!øðɔn]
- (10b) "il leur donne" [ijøðɔn]

Ainsi la règle se réécrira comme suit:

- (4) $l \longrightarrow \langle \emptyset \rangle / - [\text{syll.}] \rightarrow$ } clitiques

Cette règle rend donc compte de tous les cas possibles de chute du /l/ des pronoms: "les", "la", "le", "l' " et "leur".

Les pronoms il(s) et elle(s)

En ce qui concerne le "il", le problème se pose au niveau de la représentation sous-jacente. En effet, nous remarquons que chez 77 locuteurs le /i/ du "il" n'apparaît jamais. En revanche, on rencontre certains locuteurs, qui maintiennent la forme "il". Nous pourrions toujours proposer une règle de chute de /i/ obligatoire chez certains et facultative chez d'autres. Mais cette hypothèse ne nous satisfaisant pas, nous aimerions y voir deux formes sous-jacentes distinctes: /i:/ et /i/. Notons que déjà au 16^{ième} siècle, le /i/ de "il" et de "elle" était tombé par troncation devant consonne. En français québécois, la règle a pu se généraliser devant voyelle, du moins pour le "il". Un fait semble corroborer l'hypothèse du /i/ sous-jacent. A la troisième personne du féminin pluriel, nous avons l'alternance /ɛ:/ et /i:/, alors que nous retrouvons jamais l'alternance /ɛ:/ et */i:/.

- (11a) "elles viennent" [ivjen]
 (11b) "elles ont" [jɔ̃]
 (11c) "elles sont finies" [isɔ̃fini]
 (11d) "elles s'ennuyaient" [isənɥijɛ]

A première vue, nous pourrions croire qu'il en est de même pour le pronom "elle". En effet, 65% des locuteurs font tomber à 100% le /i/.

- (12a) "elle te recevait" [atɛrsɛvɛ]
 (12b) "elle me poussait" [ampusɛ]
 (12c) "elle était à la radio" [ɛ:tə:radjo]
 (12d) "elle avait dix-huit ans" [a:vɛdizɥitɔ̃]

Cependant, en analysant les données, nous nous rendons compte que plusieurs individus, pour qui le /i/ avait complètement disparu dans "il", le maintiennent dans "elle".

Ce phénomène se produit lorsque le pronom est suivi d'une voyelle et tout particulièrement de /a/ ou /ɛ/.

- (13a) "elle aimerait ça" [alɛmrasɔ̃]
 (13b) "elle a acheté" [alɔaʃtɛ]
 (13c) "elle a recommencé" [alɔrkɔmɛsɛ]
 (13d) "elle avait" [alavɛ]

Il nous reste donc deux solutions possibles.

- 1) ou bien le /i/ n'a pas complètement disparu et on ne peut postuler qu'une seule forme sous-jacente, /ɛ:/ . Le cas échéant, il nous faut donc une règle d'effacement du /i/ pour expliquer (12a,b,c,d).

I —→ <∅> / -]elle

Cette règle est lexicale puisqu'elle ne s'applique qu'à un seul mot.

2) ou, on a deux formes sous-jacentes, l'une en /ɛl/, l'autre en /ɛ/ ou /a/. Ainsi, le /l/ que nous retrouvons parfois à l'intervocalique a pu être introduit par épenthèse. (Ce phénomène d'introduction de consonnes est d'ailleurs très fréquent en français québécois. ex.: "ça a du goût" [salɔdygu])

Il nous est actuellement impossible d'adopter l'une ou l'autre des solutions, du moins en ce qui concerne le /ɛl/.

Ordre des règles

Troncation

Elision

Gémiation

Chute de /ə/

Chute de /l/] pronom clitique

Chute de /r/

Chute de /l/] article

Etude sociolinguistique

Lors du dépouillement, nous avons étudié 15 contextes. (Voir le tableau 1). Certains sont sans grande contraintes grammaticales;

les, la (art.) /l/ → ∅ / V-

les, la (pro.) /l/ → ∅ / V- et / C-

lui (pro.cli.),

"on lui donne" [ɔidɔn]

"Marc lui a dit" [markijɔdi]

il (s), elle (s) (pro.cli.)

d'autres avec de grandes contraintes morphologiques;

l' (art.) (Voir ex. 5a, b)

le (art.),

"j'ai organisé le club" [ʒɛrganize:klyb]

et enfin les aphérèses du type;

les, la (art.) /l/ → ∅ / ##- et / #-

"Les trois quarts appartiennent"

[etɾwɔkɔrapɛrtjɛn]

"La mère, elle nous jetait..."

[amɛr anu[te]

Lorsqu'un individu laisse tomber un /I/ dans des contextes à grandes contraintes, cela constitue un indice de plus au sujet de ses performances stylistiques. En effet, nous constatons que dans la majorité des cas, ceux qui vont au-delà de ces deux derniers types de contraintes, sont ceux qui ont un taux élevé d'effacement.

Chute du /I/ selon le sexe des locuteurs.

Après comptage de toutes les chutes, nous avons pu ordonner la population par pourcentage d'élimination pour l'ensemble des contextes. Il nous a fallu trouver des corrélations avec le sexe, l'âge, la scolarité et le statut socio-économique des individus et leurs taux d'effacement. Une tentative de traitement des données par ordinateur nous a démontré qu'une étude faite sans traitement par ordinateur, s'avèrerait plus pertinente afin d'interpréter de tels résultats.

En ce qui concerne le sexe, la constitution de graphiques nous a démontré que pris dans son ensemble, le comportement de la population ne montrait rien de significatif. Toutefois, en divisant les individus en deux grands groupes sociaux, les professionnels et les travailleurs 1, et en les étudiant séparément, il semble au moins y avoir une tendance chez les classes travailleuses qui n'apparaît pas chez les classes professionnelles.

Ainsi, les dix premiers 'travailleurs' apparaissant au début de la liste ordonnée 2, sont exclusivement des femmes. Plusieurs ont déjà tenté d'expliquer ce phénomène en prétendant que par son rôle social, la femme tend à demeurer plus conservatrice sur le plan linguistique, étant plus sensible aux préjugés sociaux construits par rapport aux styles linguistiques.

Chute du /I/ selon l'âge des locuteurs.

Il nous a fallu diviser la population et l'étudier séparément, sans quoi rien de significatif ne serait apparu. Ainsi, chez les classes travailleuses, l'âge ne semble pas jouer le moindre rôle. Tandis que, au contraire, chez les professionnels, les plus âgés semblent se distribuer parmi ceux qui ont un petit taux d'effacement. Les plus jeunes (45 ans et moins) sont plus ou moins dispersés.

1. Les professionnels sont: les détenteurs de fonctions libérales, les hommes d'affaires, professeurs et étudiants ayant au moins une 13^{ième} année d'étude.
Les travailleurs sont: ouvriers manuels, employés de services publics, ménagères et étudiants ayant au plus une 12^{ième} années d'étude.
2. liste de tous les locuteurs, ordonnée selon la moyenne d'effacement dans tous les contextes. (Du plus petit au plus grand pourcentage)

Cette tendance peut dans une certaine mesure, s'expliquer du fait que certains des locuteurs plus âgés s'expriment avec un débit plus lent, et sont moins sujets aux réductions syllabiques et effacement phonémiques.

Chute du // selon la scolarité et le statut socio-économique.

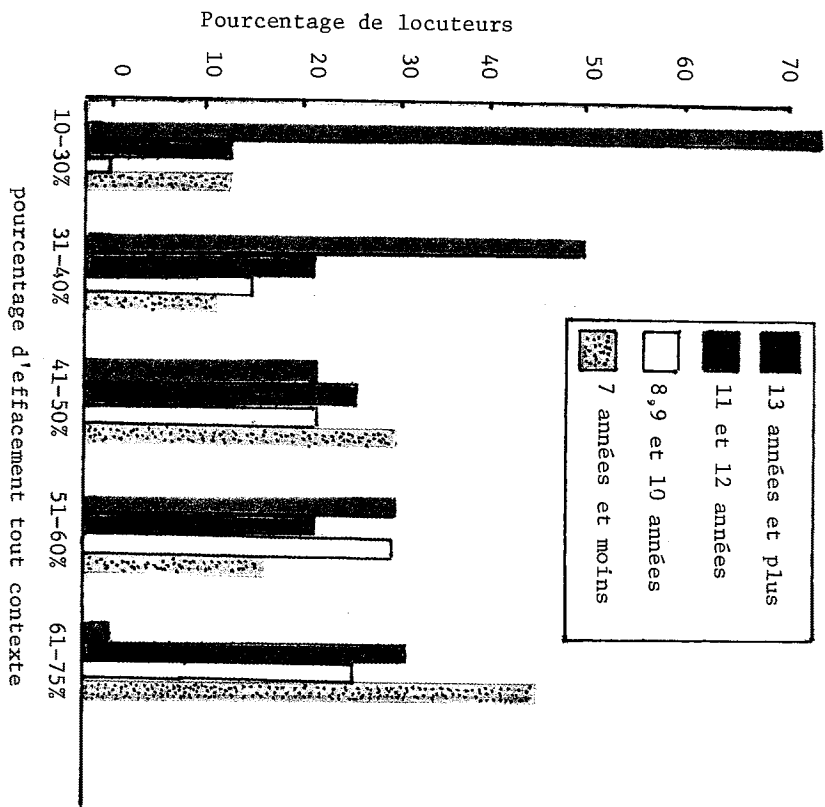
Plus un individu est scolarisé, donc jouissant le plus souvent d'un statut socio-économique valorisé par nos structures sociales, moins son taux d'élision est élevé. (Voir le graphique 1.)

Nous pouvons ainsi affirmer que l'élision du // se comporte comme un marqueur sociolinguistique. Au plan du sexe et de l'âge, elle l'est plus ou moins. Cependant, au niveau de la scolarité et du statut socio-économique, cette variable présente toutes les caractéristiques voulues.

Les professionnels, et ceux qui sont appelés à le devenir, paraissent conscients de l'existence de l'effacement du // et de ses valeurs stylistiques. Ce qui ne les empêche pas malgré tout, d'accuser eux aussi un certain taux d'élision. Doit-on attribuer certaines chutes à des lexicalisations qui n'ont plus de pertinence stylistique, et d'autres à des moments d'inattention durant l'interview? Il est vrai en revanche, que plusieurs cas de chutes inventoriés sont dus à des lexicalisations généralisées du type:

"sur la"	[sa:]
"sur les"	[se:]
"à la"	[a:]
"dans la"	[dã:]
"dans les"	[dẽ:]
"tous les"	[twe]

La conservation du // devient presque un problème personnel de style chez les individus scolarisés. Par contre, pour la majorité des franco-montréalais, l'effacement du // n'est pas un phénomène conscient et identifiable parmi toutes ces réductions syllabiques et chutes phonémiques qui composent le soi-disant français de Montréal.



GRAPHIQUE 1 : L'effacement du /l/ tout contexte selon la scolarité de 90 individus.

<u>Contexte</u>	<u>population</u>	<u>chutes</u>	<u>moyenne %</u>
Les(art) /V-	100 sujets	371/872	42.5
Les(art) /#-	100 sujets	33/890	3.7
/##-			
Les(cli) /V-	61 sujets	81/139	58.3
Les(cli) /C-	44 sujets	34/95	35.8
La (art) /V-	99 sujets	809/1376	59.8
La (art) /#-	97 sujets	17/565	3.0
/##-			
La (cli) /V-	31 sujets	15/43	34.9
La (cli) /C-	22 sujets	4/34	11.8
L' (art)	100 sujets	83/1516	5.5
Le (art)	99 sujets	42/2565	1.6
Lui(cli)	42 sujets	94/116	81.0
Elle	84 sujets	728/817	89.0
Elles	29 sujets	74/78	94.9
Il	100 sujets	3393/3446	98.5
Ils	99 sujets	1743/1765	98.8

TABLEAU 1 : L'effacement du /l/ pour tous les contextes.

BIBLIOGRAPHIE

- Laliberté, Thérèse. (1974) L'élision du "l" en français québécois. *Lingua* 33, 115-122.
- Pupier, Paul et Légaré, Luc. (1973) L'effacement du /l/ dans les articles définis et les clitiques en français de Montréal. *Glossa* 7; pp 63-80.
- Sankoff, Gillian et Cedergren, Henrietta. (1971) Some results of a Socio-linguistic Study of Montreal French. In Regna Darnell (réd.). *Linguistic Diversity in Canadian Society*. (Edmonton: Linguistic Research), pp. 61-87.

FRENCH AND ENGLISH PREPOSITIONS: A BI-STRATAL STUDY

Jean-Luc Garneau

Lake Forest College

David C. Bennett (1968) in his stratificational treatment of prepositions describes three relationships between the sememic and lexemic strata. They are:

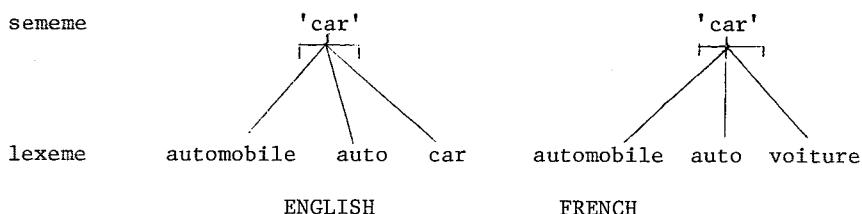
1. a one-to-one correspondence between a sememe and its lexemic realization
2. a sememe having more than one lexemic realization
3. a lexeme being the realization of two or more distinct sememes.

Studied in a bilingual situation, case 1, that of a one-to-one correspondence between a sememe and its lexemic realization can be shown thus:

sememe	'door'	'door'	'sun'	'sun'
lexeme	door	porte	sun	soleil
	ENGLISH	FRENCH	ENGLISH	FRENCH

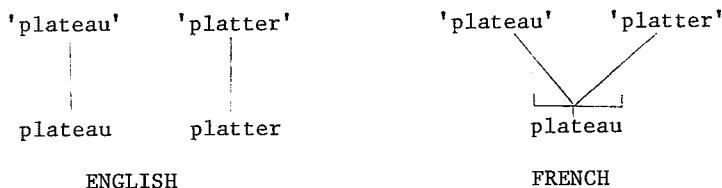
Examples of case 1 in bilingualism are called Reliable Transfer Items.

In case 2, a given sememe may be realized by more than one lexeme, as illustrated bilingually below:



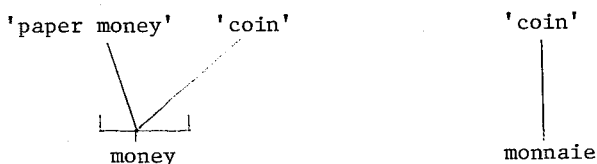
Case 3, in which a lexeme is the realization of 2 or more sememes, can be exemplified bilingually whenever either source or target language lexeme is polysemous. ¹Such is the case of what I call Borrowing for Specific Meaning, ¹a diachronic reality applied to synchrony for the purpose of identifying sememic differences between cognate lexemes. In such a case, one language adopts a polysemous lexeme from another language, but limits its sememes to

only one. For example, E. *plateau*/Fr. *plateau*:



Thus, Fr. *plateau* has greater extension than the English, which borrowed *plateau* for the specific geographical sememe, and which expresses S/'platter' by *platter*. The cognate couplet juxtaposes Misleading Transfer Items¹: they have a common sememic element, but there is not a one-to-one correspondence between lexeme and sememe in both languages. Thus Fr. *plateau* is rendered in English as either *platter* or *plateau* depending upon the sememe of the utterance in question.

Another example of a bilingual case 3 is the Misleading Transfer Item couplet *money/monnaie*:



The pair *money/monnaie* sometimes translate each other, sometimes not. *Monnaie* is only 'coin'. *Money* can also be of paper. When such is the case the translation is *argent*, which curiously is also the French lexemic realization of 'silver'. The use of *monnaie* as the realization of S/'paper money' results in a lexemic anglicism.²

But discussion of the reliability or unreliability of polysemous lexemes in translation must be based on a sound concept of what translation is. In essence, a bonafide translation is one in which all the sememes of a given utterance of the source language are realized in the lexemes of the target language utterance.

Thus, whereas at times a one-to-one correspondence will hold between S_1 and L_1 of English and between S_1 and L_2 of French, at other times and much more frequently, English L_1 realizing S_1 will be realized in French by a completely different and more lengthy set of lexemes. For example: *When the news breaks out, WAIT breaks in.* is translated *Dès qu'il se produit une nouvelle d'importance, WAIT vous la communique tout de suite.*

Furthermore, in the case of ambiguity, any meaning of the source utterance which has not been ruled out by the context must be

realized lexemically in translation, albeit by a totally separate utterance. Bennett (1969) uses the 3-way ambiguity of the sentence *The helicopter is over the hill* to illustrate his points of Progression through Space. The sentence is a prime example of meaning which, due to the absence of context, must be realized lexemically in French in 3 ways, each a bonafide translation of the English disambiguating phrase.

1. *L'hélicoptère est au dessus de la colline.*
(The helicopter is hovering directly above the hill)
2. *L'hélicoptère passe sur la colline.*
(The helicopter is in the process of traversing the hill)
3. *L'hélicoptère vole de l'autre côté de la colline.*
(The helicopter is flying on the other side of the hill)

As I will show the reasoning for later, I prefer to add a fourth disambiguating phrase and its translation) which Bennett has not treated:

4. *L'hélicoptère s'est posé de l'autre côté de la colline.*
(The helicopter is landed on the other side of the hill)

The translation of English prepositions into French is a problem whose complexities take root in the polysemy of the lexemes involved, as exemplified in Bennett's case above. I will attempt here to show in what way the polysemy of English prepositions gives rise to errors of translation into French and in so doing will isolate at least one sememe which cannot be realized prepositionally in French.

The lexotactics of French rarely permits ambiguity of prepositions³ and this results in the overt realization at the surface structure level of all of the sememes of the English preposition being translated. Thus, in some cases, for which I shall borrow the term *étouffement* 'padding' from Vinay-Darbelnet (1958), the sememes of the English preposition will be the realized lexemes at the surface structure level in French.

Given the convention in stratificational linguistics that in moving upward from the phonemic stratum toward the sememic stratum, a progression is made from expression to content or from a more concrete plane in the direction of an abstract one, then the traditionalist belief that French is more abstract and English more concrete stems from such examples of *étouffement* as follow:

The man *in* blue
L'homme *vêtu de* bleu ('dressed in blue')

I came *for* the car.
Je suis passé *prendre* l'auto. ('to take the car')

Congratulations *on* your birthday.

Félicitations *à l'occasion de* ton anniversaire. ('occasion of')

But *étoffement* is not always as clear-cut as above. There are in fact cases in which it is optional (for example, *The tree in the garden* can be translated *L'arbre dans le jardin* or *L'arbre qui est dans le jardin*) and other cases where it is obligatory if identical meaning is to be transmitted (see (4) and (5) below).⁴ Consider the following:

- (1) I am going *to* Paris.
- (2) Je vais *à* Paris.
- (3) The plane *to* Paris will leave in 5 minutes.
- (4) L'avion *à* Paris partira dans 5 minutes.
- (5) L'avion *à destination de* Paris partira dans 5 minutes.

On first glance, (1) and (2) appear to illustrate that *à* translates *to*. However, *à* in (4) is not a translation of *to* in (3), but rather means 'in', as in (6) *The plane in Paris will leave (Paris) in 5 minutes*. To translate *to* in (3), one must say *à destination de* as in (5)⁵. The *étoffement* in (5) is obligatory.

Traditionally, problems of translation as seen in (3), (4) and (5) have been regarded as merely a question of dissimilar lexotactics in French and English with little if any attention paid to the sememic strata of both source and target languages. In all of the above sentences, the lexemes in question realize the sememe 'inclination' (intended here as 'toward', the opposite of 'away'). In addition, all lexemes in question, with the exception of *à* in (4), express a change of place, Bennett's Progression through Space, a displacement. In (4), *à* is the realization of S/'at', 'in'; it is in repose, there is no change of place, only inclination without displacement.

It can be stated therefore that the S/'displacement' is realized in (1), (2), (3) and (5). I propose that this is a troublesome sememe at issue here, and that its lexemic realization in French is at the heart of numerous errors in the translation of prepositions, as well as in the translation of entire utterances, as is the case of the *chassé-croisé*.

DIAGRAM A₁

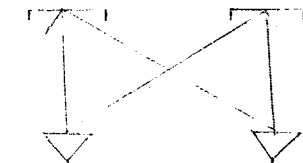
'inclination'



à

DIAGRAM A₂

'inclination' 'displacement'



aller à

à destination de

Diagram A₁ illustrates that the lexeme *à* is the realization of the sememe 'inclination', and more significantly that it does *not* realize the sememe 'displacement'. As shown in Diagram A₂, the sememe 'inclination' in conjunction with the sememe 'displacement' is realized by either a verb (*aller à*) or a prepositional phrase (*à destination de*).⁶ Thus *à* in (4) expresses no displacement, but only inclination.

DIAGRAM B (intended to explain (1) and (3))

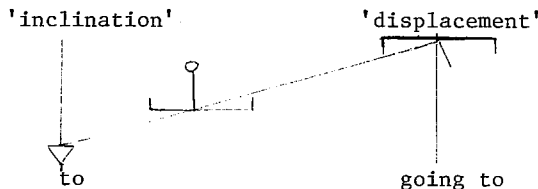


Diagram B shows *to* in (1) and (3) as the realization of sememe 'inclination' as well as of sememe 'displacement'. The UPWARD OR above *to* shows that there are cases in which *to* realizes the sememe 'inclination' without the sememe 'displacement', e.g. *Show the pictures to me* ('*Montre-moi les photos*') and in this way illustrates the polysemous nature of *to*. When S/'displacement' is present, either *to* alone or *going to* is its realization.

The following examples are offered to further illustrate that *to* can be the realization of S/'displacement' whereas the French preposition *à* cannot.

Signs on an oceanliner: (7) To the lifeboats
(8) Evacuation

Signs in a subway: (9) To State Street
(10) Côté rue State

To in both phrases (7) and (9) is the realization of S/'inclination' and S/'displacement' (*to get to* the lifeboats, *to surface on* State Street), whereas (8) by means of a substantive expresses S/'displacement', and S/'urgency' (realized in (7) by *lifeboats*) without S/'inclination'. Phrase (10) realizes S/'inclination', but clearly no displacement -- 'State Street side'. Because there is the sememic loss of 'inclination' in (8) and of 'displacement' in (10), these two cannot be considered bonafide translations, but are, instead, only equivalences. Any attempt to translate (7) and (9) into French by means of *à* would result in a sememic anglicism.

This raises the question: do French prepositions ever express S/'displacement'? I must conclude that they do not, and offer the *chassé-croisé* as evidence.

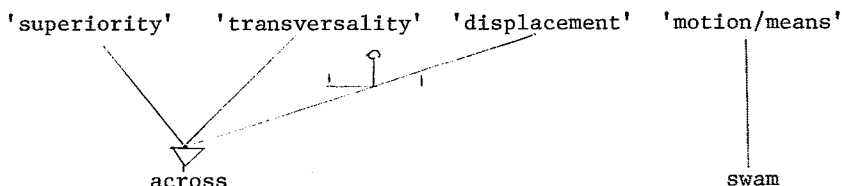
The *chassé-croisé* is a phenomenon familiar to translators and

first named by Vinay-Darbelnet (1958) in their attempt to explain the strengths and weaknesses of French and English prepositions. It consists of the flip-flopping of preposition and verb as translation takes place. More than a translator's trick, the *chassé-croisé* illustrates most effectively that the sememe 'displacement' as realized in an English preposition must be realized in French quite differently.

(11) He swam *across* the river.

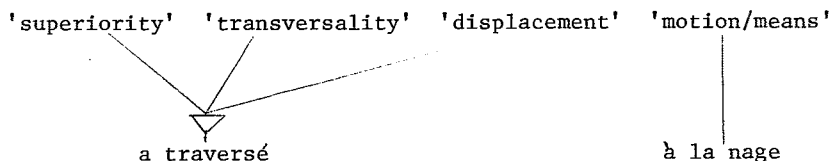
(12) Il *a traversé* la rivière *à la nage*.

DIAGRAM C (intended to explain (11))



The lexeme *across* is the realization of three sememes: transversality, superiority and displacement. *Swam* realizes the sememe 'motion/means'. It is important to note the UPWARD OR above *across* which shows its polysemy and which indicates that *across* does not always realize S/'displacement', e.g. *The tree is lying across the road* (*L'arbre s'allonge sur la route*).

DIAGRAM D (intended to explain (12))



In Diagram D, 'superiority', 'transversality' and 'displacement' are realized in the verb whereas 'motion/means' is realized by *à la nage*. Let me explain here why the verb *nager* and the preposition *à travers* cannot be used to translate *swim* and *across* in (11). The motion of swimming is translated by either *nager* or by *à la nage*, the former when no displacement is intended, e.g. *Allons nager*, the latter when a verb of displacement occurs. One way of expressing the transversality as realized in *across* is *à travers*, but this latter shows no displacement and in French therefore necessitates a verb. However, even when attempting to utilize *à travers* in conjunction with a verb of displacement, e.g. **Il a passé à travers la rivière à la nage*, the resulting utterance is ungrammatical for the feature of superiority that is also realized by *across* is not expressed by *à travers* but is in fact present in *traverser* along with S/'displacement'.

(13) He jogged *to* his office.

(14) Il *s'est rendu à* son bureau au petit trot.

DIAGRAM G (intended to explain (13))

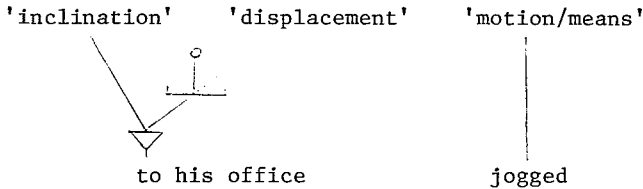
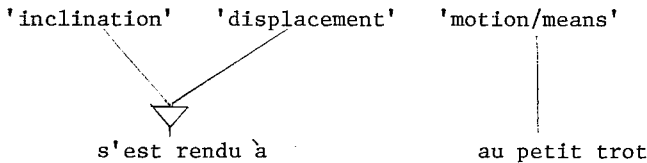


DIAGRAM H (intended to explain (14))



In Diagrams G and H, the displacement realized by polysemous *to* is translated by the verb *se rendre à*⁶ which is concurrently realizing 'inclination'.

(15) John walked *from* behind the house *over* the hill *to* the river.

(16) Jean *est parti de* derrière la maison et *s'est rendu à* la rivière *en traversant* la colline.

It is interesting to note in Diagrams I and J that *over the hill* realizes overtly the sememes 'displacement' and 'superiority' and covertly 'transversality', whereas in the French *en traversant*, it is the sememe 'superiority' which is only covertly realized.

It is not by coincidence that I have chosen to use another of Bennett's examples of the 3 phases of Progression through Space to illustrate this last *chassé-croisé*. Rather, I find that Bennett's example as seen in Diagrams I and J shows that his three fold path-expression (source, path, completion) can be reduced to no more than a triple realization of S/'displacement', which has combined 3 times with 4 separate sememes ('provenance', 'superiority', 'transversality', 'inclination') to produce the lexemic realizations in English *from*, *over* and *to* and in French *partir de*, *en traversant* and *se rendre à*. Thus from a bilingual point of view, *from*, *over* and *to* can indeed be said to be polysemous, contrary to Bennett's inferences.

It is interesting also to note the role which the sememe 'displacement' plays in Bennett's ambiguous *The helicopter is over the hill*, which was referred to earlier.

DIAGRAM I (intended to explain (15))

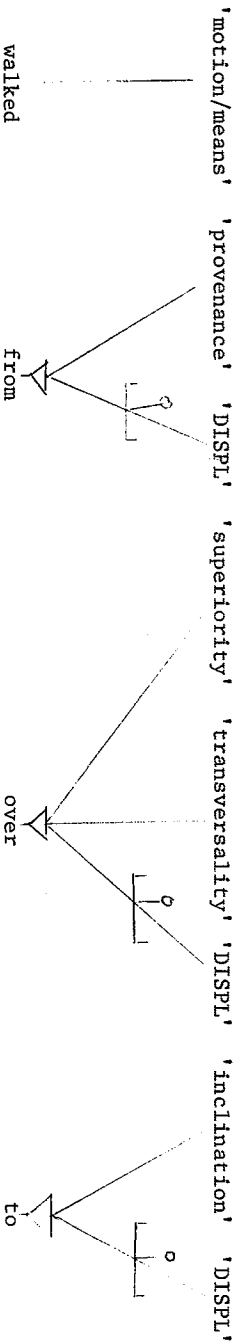
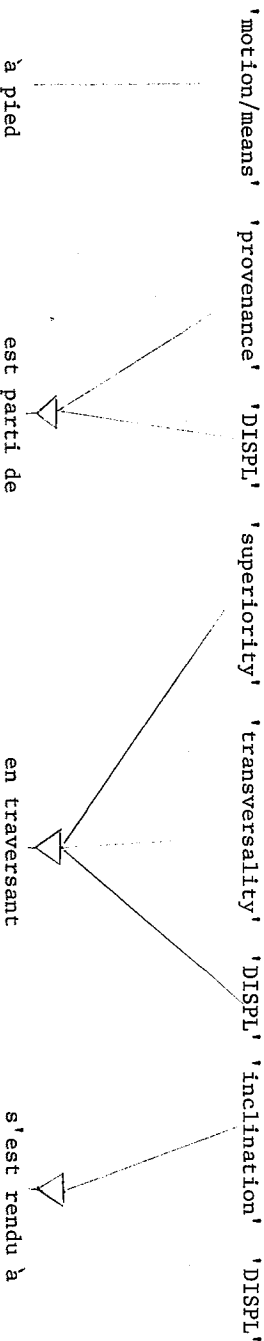


DIAGRAM J (intended to explain (16))



Phrase 1 shows the motion of the helicopter, but no displacement-*hovering*. In 2, *over* shows the motion and displacement of the helicopter - *traversing*. In 3, we have again as in 1, motion but no displacement. And in 4, neither motion nor displacement. Only 1 and 3 are in need of further explanation; the presence or absence of displacement is sufficient to distinguish 2 and 4 from each other and from 1 and 3. The distinction to be made between the meaning of *over* in 1 and in 3 is the presence in 1 of S/'superiority' and its absence in 3.

Given the above information, then, the following observations can be made concerning the bi-stratal relationship of French and English prepositions:

1. The problem of translating prepositions from English to French is greatly minimized through analysis of the semantic components of the English preposition and not, as traditionalists would hold, purely through examination of the lexotactics of each language.

2. At the lexemic level, many sememes of the English preposition are covertly realized, hence the polysemous nature which renders translation difficult.

3. The English preposition is sememically stronger, more populated than the French preposition.

4. Whereas the French preposition can also be polysemous, it will never be the realization of S/'displacement', either alone or in combination with another sememe.

5. The French preposition is sememically depleted by contrast with the English preposition, justifying in part Ullmann's contention (1952, 1953) that the entire French language is more depleted than English.

FOOTNOTES

1. cf. Garneau, Jean-Luc, The Second Lacus Forum, 1975.

2. It is not my intention to limit examples of case 3 to cognates. There exists an abundance of non-cognate Misleading Transfer Items, as any teacher of French as a second language will readily attest to.

3. The ambiguity of prepositions can occur, however rarely, as in:

Le livre sur la télévision -- *The book on the television* or
The book on television

and in:

La lettre de mon oncle -- *My uncle's letter* or
The letter from my uncle

4. When *étoffement* is optional, according to Weinreich (1961), we are dealing with vagueness; when it is obligatory, we are dealing with polysemy.

5. I recognize the possibility in this instance of translating to in (3) by *en direction de*, however in terms of Bennett's (1969) concepts of 'path' and 'completion', there is a sememic distinction to be made between the two. *En direction de* is the realization of S/'displacement' with the feature 'path', whereas *à destination de* realizes S/'displacement', with the feature 'goal'.

6. *Aller à* is a polymorphemic lexeme in which the morpheme *à* is present but not the lexeme *à*. Therefore *à* alone is not the realization of the S/inclination in this case, but rather S/'inclination as well as 'displacement' are realized by the entire polymorphemic lexeme. The same applies to *se rendre à* in Diagram H.

REFERENCES

- Bennett, David C. 1968. English Prepositions: A Stratificational Approach. *Journal of Linguistics* 4.153-172.
- Bennett, David C. 1973. A Stratificational View of Polysemy. *Readings in Stratificational Linguistics*, ed. Makkai, Adam and David G. Lockwood. The University of Alabama Press.
- Darbelnet, J. 1964. *Regards Sur le Français Actuel*. Montreal, Beauchemin.
- Garneau, Jean-Luc. 1975. Anglo-French Misleading Transfer Items: A Semantic Analysis. *The Second Lacus Forum*. South Carolina, Hornbeam Press, Inc.
- Lamb, Sydney M. 1966. *Outline of Stratificational Grammar*. Washington, D.C., Georgetown University Press.
- Ullmann, S. 1952. *Précis de Sémantique Française*. Berne.
- Ullmann, S. 1953. Descriptive Semantic and Linguistic Typology. *Word* 9.225-240.
- Vinay, J.-P. and J. Darbelnet. 1958. *Stylistique Comparée du Français et de l'Anglais*. Paris, Didier.
- Weinreich, Uriel. 1963. *On the Semantic Structure of Language*. Cambridge, Massachusetts, The M.I.T. Press.

LE RÔLE DE L'INTERFÉRENCE DANS L'EMPLOI DES PRÉPOSITIONS EN FRANÇAIS ET EN ANGLAIS PAR DES JEUNES FRANCO-ONTARIENS BILINGUES*

Raymond Mougéon, Monique Bélanger & Michael Canale

Centre d'études franco-ontariennes
Ontario Institute for Studies in Education

Introduction

Les prépositions courantes du français et de l'anglais constituent deux systèmes complexes. En se combinant avec les verbes et les substantifs, la plupart de ces prépositions prennent de nombreux sens différents pouvant parfois donner lieu à des distinctions subtiles (cf. Grevisse (1975); Quirk, Greenbaum, Leech & Svartvik (1972)). En outre les prépositions ne sont pas toujours des éléments linguistiques d'une valeur sémantique très élevée et par conséquent elles sont susceptibles d'être omises ou substituées les unes aux autres (Brown (1973); Scott & Tucker (1974)). On comprend pourquoi elles peuvent sous la pression des usagers (et en dépit des efforts des puristes) être tout particulièrement affectées par des processus de variation ou changement linguistique (Haase (1969); Gougenheim (1971)).

Dans la présente étude nous allons examiner certains des usages non standard des prépositions pour, sur et at et de la postposition back par des élèves bilingues inscrits dans des écoles de langue française de l'Ontario. Plus particulièrement nous allons étudier les usages que l'on peut expliquer (dans une mesure variable) par l'interférence. Notre étude va porter sur les quatre points suivants: (a) l'hypothèse de Weinreich (1968) selon laquelle les prépositions donnent rarement lieu à des emprunts (borrowing); (b) la constatation que dans les cas où il est plausible de supposer que l'interférence est à l'origine d'un usage non standard donné, il est souvent plausible de supposer que celui-ci est également dû à des facteurs d'ordre intrasystémique (Richards (1971); Dulay & Burt (1974); Mougéon & Canale (1977)); (c) la constatation que si l'interférence peut donner lieu à l'adjonction d'une préposition non requise à certains verbes de la langue cible (interférence lexicale), cette préposition se plie aux règles syntaxiques de la langue cible et non à celles de la langue source; (d) l'hypothèse selon laquelle il serait possible d'établir un rapport entre la fréquence d'utilisation d'une construction donnée (plus ou moins attribuable à l'interférence) et le plus ou moins grand degré de conformité pouvant exister entre certaines règles de la langue source et certaines règles de la langue cible (Mougéon, Bélanger, Canale & Ituen, 1976).

2) Les données

Notre étude repose sur des données recueillies durant trois

enquêtes socio-linguistiques réalisées auprès d'élèves bilingues de 2^{ème}, 5^{ème}, 9^{ème}/10^{ème} et 12^{ème} années inscrits dans des écoles de langue française à Welland (sud-ouest de l'Ontario), à Chelmsford et à Sudbury (moyen-nord de l'Ontario). Ces élèves ont été interviewés à deux reprises, en français et en anglais, à l'aide d'un questionnaire adapté à leurs centres d'intérêt et préoccupations, qui nous a permis d'obtenir un discours relativement spontané. Les entrevues ont duré en moyenne 40 minutes chacune. Elles ont été enregistrées puis transcrites. Nous disposons au total de 272 entretiens. Parallèlement et aussi à l'aide d'entretiens enregistrés, nous avons recueilli des données sur l'anglais d'un groupe d'unilingues torontois du même âge, ainsi que sur le français d'un groupe d'unilingues également du même âge, résidents de la ville de Québec. Ces deux groupes témoins nous aident notamment à repérer les innovations qui sont propres aux jeunes Franco-ontariens, que celles-ci soient dues à l'interférence linguistique ou non.

3) Le contexte socio-linguistique

Comme nous l'avons mentionné plus haut notre échantillon est composé d'élèves franco-ontariens inscrits dans des écoles où l'instruction est donnée en français. Le bilinguisme de ces élèves est dû à la dominance de la langue anglaise en Ontario et à la présence d'une population anglophone dans les trois communautés retenues. Cependant ces élèves sont bilingues à des niveaux différents. Ainsi, d'une façon générale à Welland et à Sudbury, les villes où les francophones sont minoritaires, dès la fin du cycle élémentaire, les élèves des écoles de langue française tendent à préférer l'anglais pour communiquer entre eux. A Chelmsford, localité où les francophones sont majoritaires, les élèves de l'école secondaire franco-ontarienne tendent à conserver le français comme langue de communication dominante. D'autre part dans les trois localités, les jeunes Franco-ontariens ont peu souvent l'occasion, excepté à l'école, d'entendre ou de voir du français canadien formel, car sur le plan communautaire, le français occupe encore une position de second plan.

4) Les prépositions et l'emprunt linguistique

Par emprunt linguistique nous entendons avec Weinreich (1968) l'incorporation dans la langue cible d'un élément linguistique signifiant de la langue source. Selon la théorie de l'emprunt linguistique énoncée par cet auteur, plus un élément linguistique est étroitement lié à un ou d'autres éléments linguistiques, moins il sera sujet à l'emprunt. C'est en particulier le cas des éléments morpho-syntaxiques comme les terminaisons, les affixes, les pronoms etc. Inversement les éléments linguistiques plus indépendants tels que les substantifs, les adjectifs ou les mots de liaison, donneront lieu plus facilement à des emprunts. Toujours selon Weinreich les prépositions et postpositions appartiennent à la catégorie des éléments linguistiques subordonnés (dépendants du verbe et des substantifs) et par conséquent ne devraient que rarement donner lieu à des emprunts. Un examen de nos données a révélé

l'absence quasi-totale de prépositions ou postpositions anglaises dans le discours français des élèves franco-ontariens, exceptions faites de la postposition back et l'absence de prépositions françaises dans leur anglais.¹ Ceci dit, l'emprunt de back, innovation typique de certains élèves franco-ontariens² (et absente du français des Québécois unilingues), mérite que nous en fassions un examen rapide. Il révèle en effet que des facteurs historico-linguistiques peuvent faciliter l'emprunt d'éléments linguistiques dépendants (et donc théoriquement résistants à l'emprunt). Donnons tout d'abord quelques exemples de cette innovation: m'as revenir back à maison; la roue, t'as r'mets back su'l'bicyc'; tu l'ramènes back où tu l'as acheté; faut lui donner back. Signalons tout de suite que la construction (re) + verbe + back alterne avec la construction équivalente sans back à savoir re + verbe, exemple: faut tu lui r'donnes. Cette alternance existe du reste sur le plan inter et intra-individuel (cf. Canale, Mougéon, Bélanger & Main (1977) pour plus de détails). Il faut y voir là le signe d'une innovation en cours d'incorporation. Ceci dit, nous pensons que l'emprunt au premier abord étonnant de back peut s'expliquer en partie par l'affaiblissement sémantique qui caractérise en français moderne le préfixe re. Cet affaiblissement se traduit notamment par l'utilisation de constructions où re (dans son sens de répétition) est utilisé concurrentement avec une périphrase telle que à nouveau, encore (une fois) etc., exemple: je le redis encore, ou même du verbe sans re suivi d'une périphrase de répétition, exemple: je le dis encore. Nous avons recueilli plusieurs exemples de ces constructions au cours de nos entrevues. Cet affaiblissement sémantique de re a du reste déjà été noté par entre autres Frei (1971) & Darmesteter (1932) qui signalent l'existence de plusieurs verbes préfixés de re, qui en français actuel ont perdu leur valeur de répétition (exemples: rentrer, recracher, refiler, relacher, etc.)³ Ceci dit re dans son sens général de retour à un état, un lieu antérieurs n'a pas d'équivalent périphrastique qui peut le remplacer dans tous les cas. La périphrase en arrière est, en effet, d'un emploi limité. C'est donc l'absence d'une véritable périphrase de remplacement pour re dans ses divers sens de retour et l'affaiblissement général de la valeur sémantique de re qui ont facilité, voire entraîné, l'emprunt de la postposition back (équivalent analytique de re dans ses différents sens de retour) et ce, en dépit du caractère dépendant de cette postposition.

En conclusion, le principe énoncé par Weinreich (1968) (relativement à la rareté des emprunts d'éléments linguistiques du type subordonné) nous semble valable d'une façon générale, cependant il convient de noter qu'à un moment historique donné, les besoins structuraux et fonctionnels d'une des deux langues en contact peuvent constituer un facteur suffisamment puissant pour entraîner l'emprunt d'un élément dépendant comme une postposition.

5) La question du rôle de l'interférence

Si nous avons trouvé peu de cas d'emprunts de pré ou postpositions dans le français et l'anglais des jeunes Franco-ontariens nous avons par contre trouvé un nombre non-négligeable de tour-

nures prépositionnelles non standard qui, d'une part étant donné leur absence du discours des deux groupes d'unilingues et d'autre part étant donné la présence de structures similaires dans l'autre langue, peuvent être attribuées en partie à l'interférence linguistique.⁴ Le problème ainsi posé, il reste à essayer d'évaluer la part jouée par l'interférence, tâche parfois difficile comme nous allons essayer de le montrer.

Considérons un instant les structures suivantes: i vient sur les fins de semaine/sur le dimanche; y'ont regardé pour une maison; j'ai vu ça sur la télévision; we're going at Montréal.

Arrêtons ici la liste, car chacun de ces exemples mériterait une longue discussion. Comme nous venons de l'indiquer plus haut, il existe en anglais et en français des structures correspondant à ces tournures non standard à savoir: on the weekends/on Sunday (Fr. st.: les fins de semaine, le dimanche); look for (Fr. st.: chercher): on television (Fr. st.: à la télévision): aller à (Ang. st.: go to). Examinons maintenant chacune des structures non standard. Le patron structural sur + unité de semaine représente une innovation qu'il paraît difficile d'attribuer à autre chose qu'à l'interférence. En effet contrairement à on la préposition sur n'est pas utilisée en français standard avec les unités de la semaine. On peut donc supposer que l'adjonction de sur là où le français exigerait la préposition pendant ou la préposition $\emptyset$ est le résultat du transfert du patron anglais correspondant. Ceci dit, on peut noter qu'en français standard, on trouve sur avec certaines expressions de temps. Ainsi on peut noter l'existence des expressions: sur le champ; sur l'heure; sur la cinquantaine; sur les deux heures. Bien que ces tournures soient peu usitées en français canadien elles témoignent d'une certaine tendance en français à utiliser sur avec certaines unités de temps. Cette tendance constitue peut-être un facteur qui a facilité ou renforcé le transfert intersystémique noté plus haut. La tournure go + at + nom de ville est doublement non conforme à l'anglais standard. D'une part l'anglais n'utilise pas at avec les noms de lieu de grande dimension (comme les villes), d'autre part il ne permet pas l'utilisation de at après un verbe de mouvement. Le français par contre utilise une préposition sémantiquement et phonétiquement proche: à, qui ne connaît pas de telles restrictions puisqu'on l'utilise indifféremment après des verbes qui expriment ou n'expriment pas le mouvement et devant des lieux de petites ou grandes dimensions, exemples: il travaille à l'hôpital/à Montréal, etc.; je vais à l'hôpital/au Canada, etc. On peut donc supposer que l'innovation go at + nom de ville (absente du discours des unilingues) est pour une bonne part attribuable à l'interférence de l'usage non-restrictif de à. Cependant, il n'est pas exclu que l'utilisation de at avec un nom de ville ne soit pas aussi le résultat partiel d'une extension analogique de l'usage normal de at devant des lieux de petites dimensions (at school; at home, etc.). Avec la tournure, regarder pour (utilisée à la place de chercher) l'interférence nous semble toujours une explication majeure. En effet regarder pour peut être logiquement considérée comme le calque du patron anglais équivalent

look for. Ceci dit, on notera l'existence en français familier du patron structural général, verbe + pour + substantif, exemples: l'autobus s'arrête pour des voyageurs; le plombier est venu pour le robinet; il fait du temps supplémentaire pour son chalet, etc. Ces structures peuvent être considérées comme le résultat de l'effacement d'un infinitif complétant le patron verbe + pour (exemple: il est venu pour (réparer) le robinet). Ici encore on peut donc supposer que le patron général verbe + pour + substantif a joué le rôle de renforcement analogique dans le transfert intersystémique noté plus haut. A ce sujet on peut remarquer que regarder pour trouver (quelque chose) constitue une interprétation sémantique possible du verbe standard chercher. Avec la tournure sur la télévision (Fr. st.: à la télévision) on peut certes invoquer l'interférence de l'expression anglaise équivalente on television. Cependant (encore plus nettement que dans le cas précédent) on peut supposer que plusieurs expressions typiquement françaises connexes ont pu également contribuer⁶ (par le biais de l'extension analogique) à la genèse de l'innovation sur la télévision. Il s'agit des expressions françaises suivantes: sur les ondes de C.J.B.C.; sur la deuxième chaîne; sur la modulation de fréquence.

Pour conclure cette section, nous espérons avoir démontré que si pour un certain nombre d'innovations linguistiques proprement franco-ontariennes il est possible d'invoquer l'interférence comme une cause probable de celles-ci, il n'en découle pas forcément qu'elle en soit la cause unique. Bien au contraire, nous avons vu qu'une analyse purement structurale des deux langues en contact nous amène à considérer l'influence possible d'autres facteurs intrasystémiques qui peuvent jouer un rôle allant du renforcement à la cause originale. Devant cette impossibilité certains auteurs (Richards (1971); Dulay & Burt (1974)) en sont venus à hautement sousestimer le rôle de l'interférence. Ainsi dans leurs études consacrées aux erreurs d'enfants apprenant l'anglais comme langue seconde Dulay & Burt (1974) font preuve d'un excès tout aussi regrettable que celui dont font montre les contrastivistes, puisqu'ils classent toutes les erreurs où l'interférence n'apparaît comme cause unique, dans la catégorie d'erreurs intra-systémiques. Sur la base de nos données et de l'analyse qui précède, nous estimons qu'une position plus réaliste consisterait à rejeter une classification dichotomique au profit d'une classification graduée (allant du plus ou moins attribuable à l'interférence ou inversement à la restructuration intra-systématique). D'autre part étant donné, le caractère intuitif de nos analyses, leur limitation à des faits purement structuraux et la complexité de la réalité linguistique à analyser, nous pensons que les études sur la causalité des innovations linguistiques en situation de contact ne peuvent aboutir à rien de plus qu'à la formulation d'hypothèses et ce seulement après un examen approfondi des règles et contraintes propres aux sous-systèmes dans lesquels s'inscrivent les innovations qui font l'objet de l'analyse.

6) Les limites de l'interférence

On a déjà remarqué, (Haugen (1950); Weinreich (1968)), que l'em-

prunt de signifiants lexicaux s'accompagne parfois de modifications qui peuvent affecter la phonologie, la morphologie, le sens et l'emploi syntaxique propres à l'élément emprunté. On a également constaté que ces modifications vont souvent dans le sens des règles de la langue cible. Bien que les tournures prépositionnelles examinées plus haut ne soient pas des emprunts lexicaux mais plutôt des cas d'interférence lexicale au deuxième degré, nous avons également observé qu'elles ont subi de pareilles modifications. Plus précisément nous avons, par exemple, remarqué que l'emploi non requis de pour après certains verbes (cf. plus haut) se pliait aux règles syntaxiques des prépositions françaises et non à celles des prépositions anglaises. Ce phénomène n'est peut-être pas sans rapport avec cette capacité de résistance de la langue (en particulier de sa syntaxe) dont parle Sapir (1949). Nous en donnons ici deux exemples tirés de nos données franco-ontariennes: ma pizza, faut tu payes pour; j'ai pas regardé pour. Nous avons conjecturé plus haut sur l'origine de l'innovation regarder pour + substantif. Nous renvoyons le lecteur à Canale, Mougeon, Bélanger & Ituen (1977) pour une analyse de la tournure payer pour + substantif. Si l'on peut admettre que les deux constructions verbales sont dues en partie à l'interférence des équivalents anglais (pay for et look for) on remarque par contre le caractère non anglais des deux exemples ci-dessus. Plus précisément dans ces deux cas précis l'anglais n'admet pas l'effacement de l'objet après for. En d'autres termes il ne permet l'emploi adverbial de for (exemples: my pizza, you'll have to pay for it; I didn't look for it). Nous n'entrerons pas dans le détail des règles qui gouvernent l'emploi ou le non-emploi de prépositions anglaises en position adverbiale. Nous nous contenterons de remarquer que ces usages adverbiaux de pour, bien qu'en apparence peut-être un peu surprenants, sont en fait conformes à la tendance du français familier à utiliser certaines prépositions (pour incluse) comme des adverbes, exemples: es-tu venu avec; je pourrai jamais faire sans, c'est étudié pour, etc. On constate donc que si l'emploi de pour après les verbes regarder, attendre et payer tire sans doute en partie son origine de verbes anglais équivalents, il n'obéit pas aux restrictions qui régissent l'emploi adverbial de for en anglais. Bien au contraire, il est conforme à l'emploi plus "libéral" des prépositions en position adverbiale, propre au français parlé. Nous retrouvons donc bien là (au niveau syntaxique) le phénomène de la restructuration de l'emploi des tournures lexicales dues à l'interférence, auquel nous venons de faire allusion plus haut.

7) Fréquence d'utilisation des innovations intersystémiques et conformité aux règles de la langue cible.

Dans une analyse consacrée à l'usage de la préposition sur par des groupes d'élèves franco-ontariens bilingues (Mougeon, Bélanger, Canale et Ituen, 1976) nous avons constaté que les innovations dans l'usage de sur (plus ou moins attribuables à l'influence de l'anglais) n'étaient pas utilisées avec la même fréquence.

Tableau 1: Fréquence d'utilisation de quelques innovations dans l'usage de sur.

Innovation	% d'emploi N. d'occurrence	Variantes Standard	% d'emploi N. d'occurrence
Sur la rue	79% 59	Dans la rue	21% 16
Sur l'avion/le train/ l'autobus	40% 18	Dans l'avion, le train, l'autobus	60% 27
Sur la radio, sur la télévision	40% 15	A la radio, à la télévision	60% 22
Sur les fins de semaine, sur le dimanche, etc.	23% 3	Les fins de semaine, le dimanche	77% 10

Les résultats du tableau 1 proviennent de la combinaison de données recueillies à Welland, Sudbury et Rayside (cf. plus haut) auprès des élèves du secondaire. Nous avons examiné deux de ces innovations dans la section 5. Voici un exemple de chacune d'entre elles: j'l'ai vu sur la rue Main; i'n'avait du monde sur l'avion; j'ai regardé la game sur la télévision hier à soir; i vient nous voir sur les fins de semaine. On peut mettre ces quatre innovations en parallèle avec des tournures anglaises équivalentes: I saw him on Main Street; there were a lot of people on the plane; I watched the game on television last night; he comes to see us on the weekends. Nous pensons que l'on peut essayer d'établir un rapport entre les différences de fréquence qui apparaissent plus haut et le plus ou moins grand degré de conformité des structures anglaises (source possible de l'innovation) à certaines des contraintes syntaxiques et sémantiques que régissent l'usage de sur. Nous allons essayer d'en faire la démonstration. Commençons par l'innovation la moins fréquente sur + fin de semaine ou jour de la semaine. Nous faisons l'hypothèse, que son taux de fréquence relativement bas n'est pas sans rapport avec ce que nous avons dit plus haut concernant l'emploi relativement rare et exceptionnel de sur avec les expressions de temps, et l'absence de structures connexes qui sont proches des patrons anglais correspondants. Plus précisément nous pensons que c'est pour ces raisons que le patron anglais on + unité de semaine n'a pas donné lieu à une innovation très fructueuse dans le dialecte des jeunes Franco-ontariens. En ce qui concerne sur la radio et sur la télévision, nous avons constaté plus haut la présence de plusieurs structures connexes conformes à l'usage anglais on + radio, television. On peut donc émettre l'hypothèse que la présence de ces structures connexes a facilité l'intégration des innovations sur la radio et sur la télévision, et dans une certaine mesure explique leur caractère relativement productif (taux d'utilisation assez élevé). Dans un même ordre d'idées on peut tenter d'expliquer le taux relativement élevé d'utilisation de l'innovation sur + train/avion/autobus, par d'une part la présence de tournures connexes où sur est utilisé avec des véhicules plus ou moins fermés, exemples: sur le bateau; sur le char, la charette, etc.; sur la remorque et d'autre part le flottement qui

existe en français actuel, relativement à l'usage de sur et dans avec certains semi-volumes, sur/dans le fauteuil, sur/dans le lit. Encore une fois on peut donc supposer que ces facteurs ont facilité l'entrée de l'innovation sur + avion/train/autobus et ont contribué à son développement. Avec la tournure sur + rue, nous avons affaire à un cas spécial dans la mesure où il n'est pas sûr qu'elle provienne de l'usage anglais on + street. En effet contrairement aux autres tournures nous l'avons aussi trouvée dans le français des élèves unilingues de Québec avec un taux de fréquence plus bas il est vrai (56%). S'il est vrai qu'en français standard le mot rue demande la préposition dans (peut-être parce qu'on peut le considérer comme un quasi-volume) il n'en demeure pas moins que la grande majorité des mots désignant une voie routière (route, auto-route, avenue, boulevard, etc.) demande la préposition sur. On peut donc concevoir l'utilisation de sur avec rue comme l'élimination d'une exception et ce indépendamment du contact avec l'anglais. Ceci dit, il n'est pas exclu, dans le cadre de la situation franco-ontarienne, que la tournure anglaise on + street n'ait pas joué et ne continue pas à jouer un rôle de renforcement, ce qui expliquerait peut-être la différence entre la fréquence d'utilisation de cette tournure par les Québécois et les Franco-ontariens et le taux très élevé d'utilisation trouvé pour les Franco-ontariens.

S'il nous paraît raisonnable d'émettre l'hypothèse de l'existence d'une relation entre le degré de conformité de certains patrons anglais et la fréquence d'utilisation des innovations correspondantes qu'ils ont pu générer, le premier ne constitue pas pour cela une explication unique du second. On devrait, à notre avis, dans le cadre d'une étude plus détaillée, essayer de tenir compte d'autres facteurs. Sur le plan linguistique, on pourrait par exemple tenter de voir si la fréquence ou l'étendue de la structure anglaise qui donne lieu à une innovation peut constituer une explication supplémentaire de la fréquence de cette dernière. D'un point de vue socio-linguistique, il conviendrait également d'essayer de connaître l'histoire du changement linguistique que représente une innovation donnée. Ainsi une fréquence d'utilisation basse trouvée pour une innovation donnée peut également en partie refléter le fait qu'elle n'en est qu'à ses débuts. Il serait bon également de voir si le degré de connaissance de l'anglais par le bilingue influe sur le fréquence d'utilisation de l'innovation. Finalement, nous pensons que notre hypothèse sur la relation entre le degré de conformité aux règles du français des innovations et de leurs sources et l'utilisation variable des innovations mériterait d'être approfondie et vérifiée par des expériences sur l'acceptabilité des innovations des bilingues par des unilingues.⁸

Notes

- * Nous tenons à remercier E. Beniak et E. Chaudeurge d'avoir fait une lecture critique de cette étude, nous permettant ainsi de l'améliorer.
1. Les cas de décrochage linguistique (switches) n'ont pas été considérés dans le cadre de cette étude, car ils relèvent beaucoup plus de l'alternance des codes que de l'emprunt linguistique.
 2. L'utilisation de back semble restreinte aux dialectes français du Nord et du Moyen Nord de l'Ontario (cf. Canale, Mougeon, Bélanger & Main (1977) pour plus de détails)
 3. Ce point précis de l'histoire du français est du reste la manifestation d'une tendance plus générale propre à l'évolution de cette langue à savoir le remplacement progressif des signifiants synthétiques par des signifiants analytiques.
 4. Nous préférons en l'occurrence le terme général d'interférence (cf. Weinreich (1968) au terme de "transfert intersystémique" parfois utilisé dans les travaux sur l'acquisition d'une langue seconde. Ce dernier terme représente un cas particulier de l'interférence et ne devrait être utilisé qu'après une analyse des faits linguistiques.
 5. Français standard.
 6. Nous utilisons volontairement le terme vague de contribuer, car dans le cas présent, il nous paraît impossible d'établir si la surgénéralisation analogique a un statut de cause ou de renforcement ou encore les deux à la fois.
 7. On peut également citer le slogan publicitaire français d'Europe: Il y a toujours du soleil sur R.T.L.
 8. Pour ne donner qu'un exemple, dans le cas où les unilingues s'accorderaient pour estimer hautement inacceptable une innovation peu conforme aux règles du français, et que cette innovation présenterait un taux d'utilisation relativement élevé, on serait autorisé à chercher d'autres facteurs explicatifs.

Références

- Brown, R. 1973. A First Language. Cambridge, Mass.: Harvard University Press.
- Canale, M., Mougeon, R., Bélanger, M. & Ituen, S. 1977. Aspects de l'usage de la préposition POUR en français ontarien: interférence et/ou surgénéralisation? Travaux de recherches sur le bilinguisme, no. 12.
- Canale, M., Mougeon, R., Bélanger, M. & Main, C. 1977. Recherches en dialectologie franco-ontarienne. A paraître dans Travaux de recherches sur le bilinguisme, no. 14.
- Darmesteter, A. 1932. Traité de la formation de la langue française. Dans A. Hatzfeld et A. Darmesteter, Dictionnaire général de la langue française. Paris: Librairie Delagrave.
- Delay, H. & Burt, M. 1974. Errors and Strategies in Child Second Language Acquisition. TESOL Quarterly, vol. 8, no. 2.
- Frei, H. 1971. La grammaire des fautes. Genève: Slatkine Reprints.
- Grevisse, M. 1975. Le bon usage. Gembloux: Editions J. Duculot.
- Gougenheim, G. 1971. Etudes sur les périphrases verbales de la langue française. Paris: Librairie A. G. Nizet.
- Haase, A. 1969. Syntaxe française du XVII^e siècle. Paris: Librairie Delagrave.
- Haugen, E. 1950. The Analysis of Linguistic Borrowing. Language, 26. 210-31.
- Mougeon, R., Bélanger, M., Canale, M. & Ituen, S. 1976. L'usage de la préposition SUR en franco-ontarien. A paraître dans un prochain numéro de La Revue canadienne de linguistique.
- Mougeon, R. & Canale, M. 1977. Minority Language Schooling in English Canada: The Case of the Franco-Ontarians. Article présenté lors de la réunion de l'Association canadienne des chercheurs en éducation (conférence annuelle du S.C.E.E.) à Frédéricton, Nouveau-Brunswick, 7-9 juin, 1977.
- Quirk, R., Greenbaum, S., Leech, G. & Svartvik, J. 1972. A Grammar of Contemporary English. London: Longman Group Ltd.
- Richards, J.C. 1971. A Non-Contrastive Approach to Error Analysis. Dans Error Analysis: Perspectives on Second Language Acquisition. C.N. Candlin ed. London: Longman Group Ltd.
- Sapir, E. 1949. Language. New York: Harcourt, Brace & World, Inc.
- Scott, M. & Tucker, R. 1974. Error Analysis and English Language Strategies of Arab Students. Language Learning, Vol. 24, no. 1.
- Weinreich, U. 1968. Languages in Contact. The Hague: Mouton & Co.

ON THE SYNTAX AND SEMANTICS
OF GERMAN MODAL VERBS: A SURFACE
STRUCTURE ANALYSIS

Hans-Heinrich Lieb
Freie Universität Berlin

0 Introduction

Modal verbs, and modal expressions in general, have always provided a fascinating object of linguistic study. This holds especially for languages like English or German, which have an intricate system of modal verbs. Since roughly 1960, the number of works devoted to German modals has been growing rapidly (cf. the Bibliographies in Raynaud 1977, Reinwein 1977), and a similar development has been taking place, with some delay, in the study of English (cf. Calbert 1975 for references). The theoretical approaches used in these studies have been quite diverse, especially in research on German modals. Not surprisingly, the recent rapprochement between linguistics and logic via model-theoretical semantics and Montague grammar has led to linguistic studies of modals using the newly provided logical tools (in particular, Karttunen 1972, Grabski 1974, Groenendijk and Stokhof 1975).

It is of course impossible in a short paper to do justice either to this vast body of research or to all intricacies of the subject matter. It seems not an unfair claim, though, that fundamental problems both of a syntactic and a semantic nature have remained open. In the present paper, some of these problems will be taken up in the framework of a 'surface syntax for semantics' as it has been proposed for 'integrational linguistics' (cf. Lieb 1977a, b). I shall largely restrict myself to modal verbs in German.

Little will be said on the delimitation of modal verbs. I shall be dealing with the modal verbs in the traditional narrow sense, roughly, können, müssen, mögen, sollen, wollen, dürfen, five of which correspond etymologically to English can, must, may, shall, and will; these verbs will be called modals for short.

I will deal with two major problems:

- (1) a. How are the meanings of modals to be conceived, and what are the major types of meanings?
- b. Given a sentence with a modal verb as a predicate, how are the meanings of the modal, the meanings of the sentence, and the syntactic structures of the sentence to be related? In particular, do different meanings of the modal require different structures of the sentence?

Question (1a) presupposes that some or all modals can be assigned lexical meanings, something that is doubtful at best for tense-forming auxiliaries. There is overwhelming empirical evidence that all modals should indeed be assigned lexical meanings (cf.

e. g. Raynaud 1977, 1), and this view will also be justified by our later discussion (Section 1).

According to a widely held opinion, modal verbs, adjectives, and adverbs should all be treated alike semantically and should all be understood on the pattern of sentential operators in modal logic. Both assumptions will be rejected for modal verbs. Their meanings will be construed as relations.

Question (1b) has been formulated as neutral between a 'generative semantics' approach (Calbert 1975, Reinwein 1977) and an 'interpretive semantics' treatment. My own proposals will be 'interpretive' though not in the framework of generative grammar.

According to another generally held view (but cf. Zemb 1976, 483), syntactic surface structures are semantically inadequate for sentences with modal predicates, and different types of meanings of a predicate require different syntactic structures. Both assumptions are refuted by my own proposals, if acceptable. Given the relational conception of meanings of modal verbs, there is a unified syntactic treatment on a surface structure basis that is semantically adequate.

In Section 1 I shall begin by a discussion of types of meanings that have been distinguished in connection with modal verbs (Section 1.1). The results will motivate a relational conception of these meanings (1.2), which in turn casts into doubt the alleged analogy between modal verbs and sentential operators in modal logic (1.3). In Section 2, question (1b) is tackled on the basis of Section 1.

Given the complexity of the subject matter, this paper can only be exploratory. No attempt has been made to achieve formal rigour. For some time to come it may be more important to reach agreement on the basic structure of a solution than to narrow it down to one particular version, however elegantly polished.

1 Meanings of modal verbs

1.1 Types of meanings

In recent literature both on German and English modals a certain type of classification has become widely accepted (not counting a few sceptics like Zemb 1976). This is a classification of 'meanings' or 'uses' of modals rather than of modals themselves. It may be exemplified for German kann by means of the following sentences:

- (2) a. Karl ertrinkt nicht. Karl kann schwimmen. - "Charles won't drown. Charles knows how to swim."
- b. Lisa bleibt hier. Karl kann schwimmen. - "Liz stays. Charles has my permission to swim."
- c. Ich weiß auch nicht genau, was er grade tut. Warte mal! Karl kann schwimmen. - "I am not quite certain either what he is doing. Wait! Charles may be swimming."

The sentence with the modal kann, or 'kann-sentence', occurs in three different sequences of sentences. Consider normal utterances of these sequences, or of their English equivalents. In these

utterances, different speech acts would be performed by uttering the part that corresponds to the kann-sentence. In a normal utterance of (2a), the utterance of the kann-sentence would be a simple assertion; in (2b), we would have a permission; and in (2c), a conjecture. The following explanation may be suggested: In each utterance, kann has been 'used' differently, a different 'meaning' of kann is involved.

It is generally felt in the literature that the 'meanings' or 'uses' of kann in cases like (2a) and (2b) group together as against its 'meaning' or 'use' in (2c). (More correctly, we should speak of the use in an utterance of (2a), or the meaning involved in such an utterance. Here and elsewhere, the abbreviated formulation will be preferred; it should always be understood as an abbreviation.) There are competing terminologies both in German and English to name the distinction (cf. Raynaud 1977, 1, for a list of terms). Best known in German may be "objektiv" and "subjektiv" (Grebe 1973, 68, 72); in English, "non-inferential" for (2a), (2b) vs. "inferential" for (2c) (Calbert 1975), and "modulation" vs. "modality" (Halliday 1970). I shall argue that the corresponding classification is both ill-founded and ill-conceived and should be replaced by two different classifications that oppose (2a) to (2b) and (2c).

Typical criteria for the received classifications are found in the two following passages translated from Raynaud 1977, 2 ("modification" refers to the uses of kann in (2a) and (2b) and "modalizer" to kann in (2c)):

'Modification' by a modal verb thus is the shifting of a process into the area of logico-grammatical modalities, i. e. possibility, necessity, permissibility, obligation etc. or of their negations.

On the other hand, if a modal verb is introduced into a sentence as a modalizer, it changes the proposition as such in the way of Admoni's (1970, 166) communicative-grammatical modality; that is, it expresses an attitude of the speaker towards the content of the sentence.

Consider (2a). What is asserted by the utterance of the kann-sentence in a normal utterance of (2a) is a physical ability of Charles. Physical ability is not a logico-grammatical category. Hence, the criterion for 'modification' does not apply. Nor does the criterion for 'modalizers'; i. e. the case is not covered.

Consider (2b). The only logico-grammatical category that could be considered is permissibility. But what we have in a normal utterance of (2b) is permissibility relative to the speaker, and because of this relativization we no longer have a logico-grammatical category in any traditional sense. Hence, the criterion for 'modification' does not apply. The same is true for the 'modalizer' criterion. Thus, (2b) is not covered, either.

Let us assume that (2c) is indeed covered. But what about

(2c') Sie sagte: [(2c)]. "She said:..."

Certainly, an utterance of (2c') no longer 'expresses an attitude of the speaker towards the content of the sentence'. But obviously, (2c) and (2c') should be covered by the same criterion. We might allow, then, for the speaker of a 'reported utterance' ("reported speaker" in Calbert 1975, Sec. 5.2).

In (2c') "reported utterance" may be taken quite literally. In other cases, we must allow for any communicative or mental act that is correctly verbalized by (part of) the actual utterance. Thus, Karl kann kommen might be uttered after sie dachte, or even separately in the context of a stream-of-consciousness novel. In this case, no utterance is reported but a mental act is verbalized by part of the actual utterance. For complete generality we should use "reported act" rather than "reported utterance", which I shall keep for simplicity's sake. By "relevant utterance" I shall understand the reported utterance if there is one, and the actual utterance otherwise. (Again, it may be preferable to speak of a relevant act and its author instead of a relevant utterance and its speaker.)

It is now possible to establish a complete analogy between (2c) and (2b) because of

(2b') Sie sagte: [(2b)]. "She said:..."

Here, too, we have permissibility relative to the speaker of a reported utterance.

Still, in an utterance of (2b) there seems to be no 'expression of an attitude of the speaker towards the content of the sentence'. But this is only apparent. In utterances of (2b) and (2b') a volitional attitude is involved:

(3) The speaker of the relevant utterance is determined, at the time of the relevant utterance, not to intervene if a certain person who is referred to as Karl in the relevant utterance gets involved in the activity of swimming some time after the time of the relevant utterance.

(This formulation turns out awkward because the time factor is explicitly accounted for.) In utterances of (2c) or (2c') we have a different attitude, which may be called evaluational:

(4) The speaker of the relevant utterance is inclined to assume, at the time of the relevant utterance, that a certain person who is referred to as Karl in the relevant utterance is involved in the activity of swimming at the time of the relevant utterance.

Cases (2b) and (2b') on the one hand and (2c) and (2c') on the other are distinguished only by the fact that the first involves a volitional attitude and the second an evaluational one.

Relating these attitudes to meanings of kann, we may say that in corresponding utterances we are confronted with attitudinal meanings of the modal. No such meaning is involved in an utterance of (2a). Here we are confronted with a non-attitudinal meaning.

The difference between attitudinal and non-attitudinal meanings should be taken as basic. It is to be expected that

they have to be construed as entities of different types. Among the attitudinal meanings we may then distinguish between speaker-restricted and neutral meanings. Speaker-restricted meanings, like the ones connected with utterances of (1c) or (1c'), necessarily involve the speaker of the relevant utterance. But this is not true for all attitudinal meanings. In particular, German wollen ("want to") has an attitudinal meaning that may or may not involve an attitude of the speaker; it is 'neutral' in this respect.

The meanings of German modals may thus be distinguished by two classifications:

- (5) a. A classification into attitudinal and non-attitudinal meanings.
- b. A classification of the attitudinal meanings into speaker-restricted and neutral meanings.

Current classifications of the meanings or uses of modal verbs are frequently ill-founded because their criteria do not apply as intended, and they are ill-conceived because the need for two separate but related classifications is not recognized. Speaker-restricted meanings of a single type, the ones that involve an evaluational attitude, are opposed to all other meanings, thus blurring two essential distinctions. We are here confronted with one of the many cases where linguistic analyses have been based on a naive ontology: 'Here's the world, and there are the opinions on it'.

Let us now consider how meanings of the various kinds may be construed.

1.2 Nature of meanings

We begin with the attitudinal meanings, again using our previous example.

The meanings of kann in (2b) and (2c) should be connected with the attitudes specified in (3) and (4). How, then, are these attitudes to be understood? We follow an early proposal made by Quine in connection with 'propositional attitudes' (1956) and construe such attitudes as relations. Our proposals, though Quinean in spirit, will modify his conception. (Actually, our proposals proceed from a suggestion that is eventually rejected by Quine. It involves 'intensional entities' such as properties, which Quine considers as 'creatures of darkness'.)

The following terminology is needed:

- (6) a. "r" for the relevant utterance.
- b. "sp(..)" for the speaker of... (or the set of speakers of.. if several are allowed). The dots may be replaced by "r" or a symbol for the actual utterance (which would eventually be needed); similarly, in (c) to (e).
- c. "charles(..)" for the set of persons referred to by Karl in..
- d. "at(..)" for the time of..
- e. "after(..)" for the time after the time of..
- f. "Swimming" for the property of being involved in the activity of swimming.
- g. "Not-Int_s" as short for "is determined not to intervene".

h. "Ass_s" as short for "is inclined to assume".

In (6d) a set of persons has been assumed to make sure that charles(..) always has a value (the empty set if there is no person). The index "s" in "Not-Int_s" and "Ass_s", which is an integral part of the symbols, will play a rôle only in Section 2.2.

Using this terminology we obtain reformulations of (3) and (4) that bring out the relational character of the two attitudes and indicates the relata:

- (7) a. Not-Int_s⟨sp(r), at(r), charles(r), Swimming, after(r)⟩.
- b. Ass_s⟨sp(r), at(r), charles(r), Swimming, at(r)⟩.

For the sake of typographical clarity, angles rather than parentheses have been used to set off the argument expressions of the constants "Not-Int_s" and "Ass_s". On the assumption that the set charles(r) has exactly one element, (7b) may be read as:

- (8) The speaker of r is inclined, at the time of r, to assume of the person referred to as Karl in r the property of Swimming for the time of r.

Note that the property of Swimming is not relativized to time; it may thus be assumed for any time.

It seems reasonable to identify the meanings of kann which are involved in the utterances of (2b) and (2c) with the relations Determined-not-to-intervene (Not-Int_s) and Inclined-to-assume (Ass_s), respectively. These relations are speaker-restricted: the first-place members of each relation must be the speaker of the relevant utterance, and all times indicated must be related to the time of the relevant utterance. In the case of neutral meanings, like the standard meaning of wollen ("want to"), we have exactly the same kind of relation, except that the two restrictions are lifted. Assume, for example, an utterance of "Karl will schwimmen." in which the want-to meaning of wollen is involved. We then have the following attitude:

- (9) Want⟨charles(r), at(e), charles(r), Swimming, after(e)⟩.

"At the time of e, the person referred to as Karl in r wants of that person [himself] the property of Swimming for a time after the time of e". e is a mental process or state in charles(r), and at(e) = at(r) because of the present tense form will; for an utterance of "Karl wollte schwimmen.", at(e) would be earlier than at(r). The attitude is self-centered (the first and third arguments are identical) because r is an utterance of a sentence in which a form of wollen is construed with an infinitive. If the form is construed with a daß-clause, there is no such restriction: "Karl will, daß Emil schwimmt."

In short, both speaker-restricted and neutral meanings may be identified with relations that represent attitudes. The speaker-restricted meanings differ from the neutral ones by an additional condition on the members of the relations.

Let us now consider the non-attitudinal meanings involved in utterances of (2a). Here, too, we are confronted with a relation: a relation of physical ability that involves Charles; the time

of an event *e* which may be identified with the relevant utterance because of the present tense form kann; and the property of swimming; Charles is at that time physically able to be involved in the activity of swimming. Put differently, the ability relation holds between Charles, the time of the event, and Swimming:

(10) Able $\langle \text{charles}(r), \text{at}(e), \text{Swimming} \rangle$.

We tentatively identify the non-attitudinal meaning of kann in (2a) with the ability relation Able.

Generalizing from these examples we arrive at the following conception:

- (11) a. Both the attitudinal and the non-attitudinal meanings of modals are relations.
- b. Both an attitudinal and a non-attitudinal meaning is a relation that always includes a property among its relata.
- c. A speaker-restricted meaning is a relation that always includes the speaker and the time of the relevant utterance among its relata.

From the point of view of modal logic the conception summarized by (11) may seem objectionable.

1.3 Modal verbs and logic

Condition (11a) may seem problematic in connection with non-attitudinal meanings that are traditionally accounted for in modal logic. Suppose we do have a clear idea of logical possibility as expressed in natural languages. Logical possibility should then be associated with German kann as one of its meanings. By (11a), it must be a relation similar to the ability relation:

(12) Poss $\langle \text{charles}(r), \text{at}(e), \text{Swimming}, \text{at}(e) \rangle$.

"The relation of logical possibility holds between the person referred to as Karl in *r*, the time of *e*, the property of Swimming, and the time of *e*". Put differently: it is logically possible at the time of *e* that Charles has the property of Swimming at the time of *e*. Because of the present tense form kann, the event (process, state etc.) may be identified with the relevant utterance. Differently from the ability relation, Poss involves two intervals of time, one for the time of the logical possibility and one for the time of the swimming; in the case of "Karl kann geschwommen haben.", the second "at(*e*)" in (12) would have to be replaced by "before(*e*)".

In modal logic, logical possibility is accounted for not by relational constants like "Poss" but by sentential operators like the diamond $\Diamond$, which are put in front of a sentence to form a new sentence. A treatment along these lines may be adequate in natural languages for modal adverbs like possibly, at least in some of their uses. It is inadequate, at least syntactically, for modal adjectives like possible. (Note that it is possible that - the usual translation for the logical possibility operators - is not a constituent of a sentence such as it is possible that he

is alive; even worse, it is possible has an internal structure in which possible must be taken as a complement of the predicate is; cf. that he is alive is possible.) Treating a modal verb like a sentential operator of logic is inadequate both syntactically and semantically. Indeed, sentences with modal verbs (or adjectives) and corresponding sentences with modal adverbs are, as a rule, not semantically equivalent (cf. Newman 1976 for discussion). Modal verbs, adjectives, and adverbs must of course be treated in a way that brings out the semantic relationships between corresponding sentences; but this does not mean that there has to be a unique way of dealing with all of them.

Conditions (11a) and (11b) jointly guarantee that all modal verbs have their most essential common feature, their 'truth-value effect', which hardly a study fails to notice. Suppose that we have an utterance of a sentence with a modal verb as a predicate. Then whatever is referred to by the subject of the sentence in the utterance may or may not be as specified in the sentence. For instance, in the case of (10), Charles may or may not, at the time of *e*, be involved in the activity of swimming; still, the ability relation holds. Similarly for all attitudinal meanings. Only in the case of an appropriate necessity relation may we conclude that Charles actually has the property specified.

It is inclusion of a relevant property among the *relata* that is the essential logical point of a Quinean treatment of attitudinal meanings. This treatment has here been generalized to non-attitudinal meanings. It allows us, among other things, to do without a kind of entity that has always loomed large in the logical treatment of propositional attitudes and modal contexts, viz. propositions.

Let us now turn to speaker-restricted meanings of modal verbs. Such meanings, whether conceived as in (11c) or in some other way, cannot be accounted for by traditional modal logic. This is excluded by the essential relativization to a speaker. For the evaluational meaning of English *may*, the unaccountability has recently been pointed out by Karttunen (1972). Karttunen and Groenendijk & Stokhof (1975) suggest different ways to get around the difficulties and achieve a logical reconstruction of evaluational meanings. In my view, both attempts have failed. (Some, though not all of Groenendijk and Stokhof's criticism of Karttunen 1972 is valid; and some of the shortcomings of Groenendijk and Stokhof 1975 are correctly pointed out in Newman 1976.) Grabski 1974 does not treat the evaluational meanings as speaker-restricted, with a few exceptions; and his approach is seriously limited by a deliberate methodological restriction: only present tense infinitives are allowed, and all modal verb predicates have to be in the third person singular present if they are in the indicative. This way, the interplay of time and modality cannot be discussed, and the problems of the reported utterance cannot be treated. (Like the other logical treatments, Grabski's deserves more detailed discussion than is possible in the present context.)

Attitudinal and non-attitudinal meanings of modals will tentatively be assumed as indicated above. The conception is not

yet explicit in its treatment of reference, especially with respect to the traditional de re - de dicto problems. I would eventually adopt the semantic framework proposed in Lieb forthc. a; the present conception of meanings is only a first step towards a more adequate treatment within that framework. Still, the present conception should correctly outline the structure of possible solutions if not their precise form.

In Section 2 I shall indicate how this conception of meanings meshes with the conception of syntactic surface structure proposed in integrational linguistics (for details, cf. Lieb 1977b; for an explanation of the basic approach, Lieb 1977a; for a summary of essential notions Lieb 1976, Sec. 2.1).

2 A surface syntax treatment of sentences with modal verbs

2.1 Syntactic structures

So far we have been speaking of modal verbs without assigning them any formal status. Differently from English may etc. most German modals form complete paradigms. A German modal verb is always to be understood as a verb paradigm not as a single form; thus, the form können has to be distinguished from the verb paradigm können^P.

More precisely, we shall not consider German but the systems S of arbitrary idiolects in an unspecified variety of Modern German (cf. Lieb 1977b for justification). All linguistic entities will be relativized to these systems. Thus, we speak of können^P(S), the paradigm of können in S, where S may be any one of these idiolect systems. Syntactic categories are relativized in the same way.

A verb paradigm is a relation between syntactic units and 'type 1 marking categories'; thus

- (13) können^P(S) =
 { {kann, {1P(-, S), Sg(-, S), Ind(-, S), Pres(-, S), Act(-, S) } },
 { kannst, {2P(-, S), Sg(-, S), Ind(-, S), Pres(-, S), Act(-, S) } } },
 ⋮
 { }.

1P(-, S) etc. are sets of syntactic units of S: the set of first person verb forms etc.

Some modals may have to be taken as proper subparadigms of other modals. Thus, dürfte(S) is the preterite subjunctive paradigm of dürfen(S). There are semantic reasons for taking dürfte^P(S) as a separate modal verb of S; as a verb paradigm, dürfte^P(S) is defective. In the same way, there may be a modal möchte^P(S) that is a subparadigm of mögen^P(S).

Alternatively, dürfte^P(S) and, possibly, möchte^P(S) could be taken as auxiliaries in the narrow sense. A sequence like dürfte kommen ("might come") would then be treated as a composite form of the verb kommen(S), a form that is assigned to a certain mood; similarly, for a sequence such as möchte kommen. Thus, dürfte^P(S) and möchte^P(S) would be treated along the lines of würde^P(S), which is used to form the periphrastic subjunctive of the preterite

and pluperfect. (It has been a much debated question whether a present indicative form of werden (S) followed by an infinitive - wird kommen - is a modal verb form or an auxiliary form used for obtaining a periphrastic future tense; cf. e. g. Vater 1975. This may well be a badly formulated alternative. The form of werden (S) together with the infinitive may indeed belong to the paradigm of the infinitive. However, use of werden (S) may indicate a certain mood rather than a tense.) All other modals are clearly distinct from auxiliaries in this sense.

Just as any other non-auxiliary, a modal has properties of valence and government (see also Raynaud 1977, 5). All modals are two-valued like transitive verbs but have different properties of government. (Some modals, like können (S) and wollen (S), could also be classified as transitive verbs, though not for the reasons given in Ross 1969.) The following category is most important among the categories of modals that are determined by government:

- (14) $NOM+INF\langle -, \underline{S} \rangle$ = the set of two-valued verbs (verb paradigms) of S that require a nominal group in the nominative and an infinitival group without the particle zu.

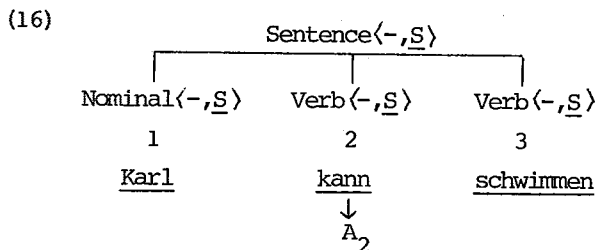
Only modals belong to this category; actually modals in the narrow sense may be identified with the verbs of this category. If the relevant paradigms of mögen (S), dürfen (S), and werden (S) are treated as mood forming auxiliaries, (14) comprises just mögen (S), wollen (S), sollen (S), können (S), dürfen (S), müssen (S), and brauchen^P (S) (which need not be combined with a negation as is usually claimed; cf. er braucht nur kommen). (14) is a 'type 2 marking category'. Such categories are sets of paradigms not forms. The significance of type 1 and type 2 marking categories for modals will become clearer when we turn to syntactic structures.

Consider the following sequence of forms (the precise nature of these forms may be left open):

- (15) $f = \text{karl kann schwimmen} = \{ \langle 1, \text{karl} \rangle, \langle 2, \text{kann} \rangle, \langle 3, \text{schwimmen} \rangle \}$.

This is a syntactic unit of S. A syntactic structure of any unit of S is a triple consisting of a constituent structure, a marking structure, and an intonation structure.

For f we assume just a single constituent structure and marking structure, as indicated in the following diagram:



where $A_2 =$
 $\{\{1P\langle -, \underline{S} \rangle, Sg\langle -, \underline{S} \rangle, Ind\langle -, \underline{S} \rangle, Pres\langle -, \underline{S} \rangle, Act\langle -, \underline{S} \rangle\},$
 $\{NOM+INF\langle -, \underline{S} \rangle, \dots\},$
 $\{3P\langle -, \underline{S} \rangle, Sg\langle -, \underline{S} \rangle, Ind\langle -, \underline{S} \rangle, Pres\langle -, \underline{S} \rangle, Act\langle -, \underline{S} \rangle\},$
 $\{NOM+INF\langle -, \underline{S} \rangle, \dots\},$
 $\vdots$
 $\dots\}.$

Nominal $\langle -, \underline{S} \rangle$ (the set of nominal forms of $\underline{S}$), Verb $\langle -, \underline{S} \rangle$ (the set of verb forms), and Sentence $\langle -, \underline{S} \rangle$ are syntactic constituent categories of $\underline{S}$. The upper part of (16), including the digits, denotes the constituent structure of f , i. e. the following relation k between sets of numbers and constituent categories:

- (17) $k = \{\{1\}, Nominal\langle -, \underline{S} \rangle\}, \{2\}, Verb\langle -, \underline{S} \rangle\}, \{3\}, Verb\langle -, \underline{S} \rangle\}, \{1, 2, 3\}, Sentence\langle -, \underline{S} \rangle\}.$

In the lower part, the marking A_2 of the constituent $\{2, \underline{kann}\}$ has been indicated. A_2 is obtained in an obvious way via the verb paradigm können ($\underline{S}$). The marking structure μ of f is obtained from A_2 and the markings A_1 and A_3 of the two other primitive constituents. (I have no space here to show precisely how μ is obtained.)

Let $s = \langle k, \mu, I \rangle$ be a syntactic structure of karl kann kommen in $\underline{S}$, where I is an appropriate intonation structure that represents a 'statement intonation'. (The nature of intonation structures will be left unspecified.) We may then assume the following syntactic relations as holding on f and s :

- (18) a. $\{1, \underline{karl}\}$ is a subject of $\{2, \underline{kann}\}$ and $\{3, \underline{schwimmen}\}$ in f , s , and S .
 b. $\{2, \underline{kann}\}$ is a predicate of f in f , s , and S .
 c. $\{3, \underline{schwimmen}\}$ is a complement of $\{2, \underline{kann}\}$ in f , s , and S .

These syntactic relations (which cannot here be justified) play an essential part in the determination of the sentence meanings.

2.2 Translations

Sentences with modals will be characterized semantically not by showing how their meanings are obtained from meanings of their parts (direct semantics) but by indicating how translations of the sentences can be obtained from translations of their parts (translational or indirect semantics). Although I consider the direct approach as ultimately superior, the translational approach is better suited for demonstration purposes.

The language into which the sentences will be translated is to be a constructed language of logic, at least in principle; it will not here be characterized in any detail. The logical language is assumed to be interpreted to the extent that the translational equivalent of a natural language expression is 'semantically representative of that expression'.

The translation consists of two parts: a syntactic paradigm

translation and a syntactic constituent translation which is based on the paradigm translation. We shall restrict ourselves to a single example, translation of (15) karl kann schwimmen. The syntactic framework will be assumed as characterized in Section 2.1, and the translations will presuppose the semantic conception of Section 1, in particular, the meanings assumed for utterances of (2).

The paradigm interpretation is to contain the following part:

- (19) a. karl^P(S) translates into "charles".
 b. können^P(S) translates into (at least) "Able", "Not-Int_S", and "Ass".
 c. schwimmen^SP(S) translates into (at least) "Swimming".

The translational equivalents are to be understood as indicated in connection with (5) and (10). Formally, "charles" is a functor, "Able", "Not-Int" and "Ass" are relational constants, and "Swimming" is a one-place predicate constant. In agreement with Section 1.2 we shall here assume that the meanings of these expressions are not only similar to but identical with meanings of the corresponding paradigms. (For the problematic nature of such an assumption, cf. Liep 1976b, Sec. 5.2.)

Neither for können^S(S) nor for schwimmen^P(S) has the paradigm translation been completely specified in (19). It is frequently claimed (e. g. by Raynaud 1977) that the evaluational meaning represented by "Ass" cannot occur with all forms of können^S(S). If this is correct (and I am not sure it is), "Ass" would be a translation not of können^P(S) but of a defective verb können₁^P, that is a subparadigm of the non-defective können^S(S).

The constituent translation is relativized to a syntactic unit and structure of the unit. For the unit $f = \text{karl kann schwimmen}$ and the structure s partly characterized in (16), the following translations are assumed:

- (20) a. $\{ \langle 1, \text{karl} \rangle \}$ translates in f and s into "charles".
 b. $\{ \langle 2, \text{kann} \rangle \}$ translates in f and s into "Able", "Not-Int_S", and "Ass".
 c. $\{ \langle 3, \text{schwimmen} \rangle \}$ translates in f and s into "Swimming".
 d. karl kann schwimmen translates in f and s into
 α . "Able(charles(r), at(e), Swimming) & $r = e$ ".
 β . "Not-Int_S(sp(r), at(r), charles(r), Swimming, after(r)) & ...".
 γ . "Ass_S(sp(r), at(r), charles(r), Swimming, at(r)) & ...".

" r " and " e " in (20d) may be understood as constants that ambiguously refer to any relevant utterance of f and some unspecified event (process state etc.), respectively. The dots in (20d β) indicate an unspecified part of the translation that is a condition on the temporal relationship between r and some other utterance, in most cases, the actual one. I deliberately refrain from going into the problems connected with " r ", " e ", and the dots, and I leave it undecided precisely how the translations are to be formally interpreted. For the purpose of this paper, the interpretive hints in Section 1 should be sufficient.

Obviously, the translations of the entire sentence f are based on the translations of the primitive constituents, which in turn are based on the translations of corresponding paradigms (19). Not all translations of the sentence and its constituents are specified in (20) because only for the incompleteness of (19) with respect to the translations of paradigms.

Let us now informally establish how the translations in (20) may be arrived at.

The translations of the three primitive constituents are simply the translations of the corresponding paradigms.

The translations of the entire sentence are obtained from the translations of the primitive constituents by taking into account:

- (21) a. The syntactic relations that hold in the sentence given its structure.
 b. Conditions on either (α) or (β) :
 α . the syntactic structure;
 β . the translations of constituents.

This can be seen as follows.

All translations are conjunctive formulas of the constructed language that consist of two relational formulas. Such translations are appropriate only because the predicate constituent $\{\{2, \text{kann}\}\}$ is 'marked' as Ind (indicative) in the syntactic structure (cf. (18a) and (16)); this is a condition as specified in (21b α). If the predicate constituent had been marked by a mood other than the indicative, either the entire translation or its first conjunct would have to be introduced by a sentential operator (provided that we decide to account for moods by sentential operators in the translation). Another necessary condition for translations as in (20d) is an intonation structure that represents a 'statement intonation'.

Let us now turn to the first conjunct. The predicate constant in this formula is one of the translations of the predicate constituent. "Able" may be chosen only because the complement constituent $\{\{3, \text{schwimmen}\}\}$ is marked as Pres (present - not indicated in (16)). Such a translation would be ruled out for Karl kann geschwommen haben. This is another condition (21b α).

"Able" is a three-place constant (condition (21b β)). The three argument expressions of the complete formula with "Able" are obtained as follows: The first argument expression is the translation of the subject constituent $\{\{1, \text{Karl}\}\}$ supplemented by "(r)". The second argument expression is "at(e)" for the following reasons: "at" is used because "Able" is a translation of a modal verb predicate (cf. (14) and A_2 in (16)). "e" is used rather than "r" because "Able" does not contain the index "s" (a condition (21b β)). The third argument expression is simply the translation of the complement constituent $\{\{3, \text{schwimmen}\}\}$.

The first conjuncts of (20d β) and (20d γ) are determined in a similar way. Only the translation with "Ass_s" will here be discussed.

"Ass" is a five-place relational constant (a condition (20b β)).^s The first argument expression is "sp(r)" because "Ass_s"

contains the index "s" (condition (20b β)). The second argument expression is "at(r)" for the same reason. The third and fourth argument expressions are identical with the first and third argument expressions of (20d α) and are obtained in the same way. The fifth argument expression is "at(r)" because the complement constituent $\{\{3, \text{schwimmen}\}\}$ is marked as Pres (not indicated in (16)) and "Ass" contains the index "s".

Now consider the second conjuncts in the three translations. "r = e" in the "Able" translation may be chosen because the predicate constituent is marked as Pres. (For karl konnte schwimmen, the second conjunct would be "Earlier{e,r}", "e is earlier than r".) The unspecified conjuncts in the "Not-Int" and "Ass" translations would also be determined by the tense marking of the predicate constituent and would relate the time of the relevant utterance to the time of another utterance.

Determination of this utterance, which must ultimately be related to the actual one, requires a careful consideration of the relation between tenses and time both in direct quotes and indirect speech. The same holds for determining de dicto and de re references of the subject constituent. It is at this point that I shall simply break off discussion.

2.3 Conclusion

I have informally described how the sentence karl kann kommen can be translated in a way that takes account of several of its meanings. In my informal description I implicitly appealed to general rules for translating sentences with modal predicates simply on the basis of their surface structures. Such rules were appealed to whenever I justified a certain feature of the translation. For more than one reason it was not possible to give an explicit formulation of the rules. Still, my analysis of a non-trivial example should support the following claims (formulated not for translations but directly for meanings):

- (22) a. The semantic ambiguity of sentences with modal verbs as predicates is due to lexical ambiguity of the verbs.
- b. All meanings of a sentence with a modal-verb predicate can be determined on the basis of syntactic surface structures that differ only in their intonational components and whose shared constituent structure component is essentially as in (16): where the modal verb constituent alone forms the predicate and both the nominal and the infinitival constituents are predicate complements.

To the extent that this has been supported, (22) destroys the basis for the current conviction (Calbert 1975, Raynaud 1977) that the semantic treatment of German modals requires 'deep structures', and different deep structures to correspond to sentence meanings of different types. It further destroys the basis for the universally held view that some or all German modal verbs should be conceived, either syntactically or semantically, in analogy to sentential operators of modal logic. The constituent structure for sentences with modal verbs as predicates suggests

relations as meanings of modal verbs, and such a conception can be justified on independent semantic grounds.

It may be expected that an investigation of modal verbs in many languages other than German will lead to similar results - certainly in languages like English or French where verbs combine with modal meanings in a similar way.

REFERENCES

- Admoni, W. (1970), Der deutsche Sprachbau, 3rd ed, München:Beck.
- Calbert, J. P. (1975), "Toward the semantics of modality", in Calbert/Vater (1975), 1-70. [Revised version of a 1971 paper.]
- Calbert/Vater (1975), Aspekte der Modalität, Tübingen: TBL Verlag Gunter Narr (Studien zur deutschen Grammatik 1).
- Grabski, M. (1974), Syntax und Semantik der Modalverben in Aussagesätzen des Deutschen, University of Stuttgart, Ph.D. diss. [mimeo].
- Grebe, P. (1973), "Zum Gebrauch der Modalverben", in P. Grebe et al., Duden: Grammatik der deutschen Gegenwartssprache, 3rd rev. and enl. ed., Mannheim etc.: Bibliographisches Institut (= Der Große Duden 4), 67-74.
- Groenendijk, J., Stokhof, M. (1975), "Modality and conversational information", Theoretical Linguistics 2 (1975), 61-112.
- Halliday, M.A.K. (1970), "Functional diversity in language as seen from a consideration of modality and mood in English", Foundations of Language 6, 322-361.
- Karttunen, L. (1972), "Possible and must", in J. P. Kimball (ed), Syntax and Semantics I, New York etc.: Academic Press, 1-20.
- Lieb, H. (1976), "Grammars as theories: the case for axiomatic grammar (Part II)", Theoretical Linguistics 3, 1-98.
- Lieb, H. (1977a), "Surface syntax as a basis for semantics", in R.J. Di Pietro, E. L. Blansitt jr. (eds.), The Third Lacus Forum 1976, Columbia, S.C.: Hornbeam Press, 405-412.
- Lieb, H. (1977b), Outline of integrational linguistics: Preliminary version, Berlin (West): Freie Universität Berlin, Fachbereich 16, (= IAB [Linguistische Arbeiten und Berichte] Berlin (West) 9).
- Newman, J. (1976), "Remarks on 'Modality and Conversational Information'", Theoretical Linguistics 3, 281-286.
- Quine, W.V.O. (1956), "Quantifiers and propositional attitudes", Journal of Philosophy 53, 177-178. [Frequently reprinted.]
- Raynaud, Franziska (1977), "Noch einmal Modalverben!", Deutsche Sprache 5, 1-30.
- Reinwein, J. (1977), Modalverb-Syntax, Tübingen: TBL Verlag Gunter Narr (Studien zur deutschen Grammatik 6).
- Ross, J. R. (1969), "Auxiliaries as main verbs", in W. Todd (ed.),

Studies in philosophical Linguistics. Series One.

Evanston, Ill.: Great Expectations, 1969, 77-102.

Vater, H. (1975), "Werden als Modalverb", in Calbert/Vater (1975), 71-148.

Zemb, J.-M. (1976), "Muß etwas möglich sein, kann etwas notwendig sein?", in U. Engel, P. Grebe (eds), Sprachsystem und Sprachgebrauch. Festschrift für Hugo Moser zum 65. Geburtstag. (= Sprache der Gegenwart 34), Düsseldorf: Schwann, 473-483.

AN EXPERIMENT ON FOCUS

Christine M. Andrew

University of Toronto

An experiment on focus is reported, in which subjects were asked for direct judgements of the importance of words in a set of twenty sentences which varied systematically according to voice, dative position, and location of contrastive stress. The results indicated that front-shifting and contrastive stress each serves to focus sentence constituents.

This experimental study was conducted in order to examine the possible focusing role of contrastive stress and word order in sentences. "Focus" is here used as a general term, roughly equivalent to "importance." Thus, to focus a sentence constituent is to make it semantically prominent. This increase in semantic prominence is a basic property of focus, and is implicit in the theories discussed below.

The focus role of word order was investigated here by using dative sentences which varied in voice and dative position. Passivization and dative movement are of particular interest because they originally motivated much of the theoretical treatment of focus. Fortunately, variations in both voice and dative position can be combined in dative sentences, along with variations in contrastive stress. Therefore, it is possible to determine not only whether each of these variations involves a change in focus, but also the relative size of their focus effects.

Theories of Focus

The notion of focus as semantic prominence was discussed by Halliday (1967, 1968, 1970) as part of his theory of discourse structure. Halliday believed that the basic unit of language is a "text", and he discussed two ways in which clauses are organized so that they are relevant to the context.

The first of these aspects of clause organization is thematic structure. The theme of a clause represents what the speaker is talking about, and is placed in initial position in the clause. The theme can therefore be viewed as the psychological subject; the remainder of the clause is the body of the message. Halliday (1968,p.212) regarded the theme as semantically prominent.

Secondly, a clause is organized within a text in terms of given and new information, in such a way that the new information is emphasized. Halliday termed this kind of organization "information structure." Information structure is treated in terms of intonation; an information unit is realized as a tone group, and its point of information focus, the new information, is indicated by the placement of the tonic.

Strong stress is required in order to designate any constituent as the specific focus—that is, the only new information in the information unit. Halliday indicated that in fact this strong stress must be contrastive in order to indicate specific focus on any lexical item except the last one, and in such cases he considered the focus to be "marked." Thus, strong stress or contrastive stress increases the semantic prominence of a constituent by making it the specific focus.

The influence of Halliday's work in the area of information structure is clearly seen in Chomsky's account of surface focus. Chomsky (1971) saw focus as a phonological phenomenon, and defined focus for sentences with normal intonation as "a phrase containing the intonation centre of the sentence." In the case of contrastive stress, focus would be defined similarly, perhaps with the restriction that focus might have to be only a short phrase containing the intonation centre. This notion is similar to Halliday's "specific focus."

Focus was also discussed by Chafe (1970) as part of a more semantically based grammatical theory. Again, the influence of Halliday's theory of information structure is evident. Chafe discussed focus in terms of old and new information. Like Halliday and Chomsky, Chafe regarded focus as semantic prominence, characterized by increased pitch and amplitude of the focused element. Like Halliday, he regarded the focused constituents to be the new information in the message.

According to Chafe (p.222), only the surface subject of a sentence normally conveys old information. In Chafe's model, then, one function of the passive is to alter the distribution of old and new information in the sentence, with one result being that the agent conveys new information and is therefore focused.

Chafe (p.224) indicated that contrastive stress can mark any element as new information, and that such new information "has been selected by the speaker from various alternatives." Like Halliday, he considered contrastive sentences to have a more marked distribution of information than noncontrastive ones.¹

Lockwood (1972) discussed focus as a semological phenomenon within a stratificational model. He did not define focus in semantic terms, but implicit in his account is the notion that focus means emphasis. Lockwood discussed two such kinds of emphasis. One is realized as the surface subject of the sentence,

which must be a nominal construction. However, Lockwood maintained that for dative sentences, the agent in surface subject position is not necessarily emphasized, while either the goal or recipient in this position must be emphasized. The second kind of emphasis which Lockwood discussed is realized as the clefted element in sentences such as "It was John who gave the book to Andrew." Such emphasis appears to be related, via Halliday's concept of information structure, to phenomena of stress. However, Lockwood did not include stress in his discussion.

Some empirical questions

These theories lead to several empirical questions. First, there is agreement that contrastively stressing an element should signal its increased semantic prominence. The more interesting question, from the viewpoint of these theories, is whether the order of the constituents in a sentence is related to their semantic prominence, and whether this is entirely dependent on the intonation patterns that these word order variations involve. Further, if word order variations do reflect differences in semantic prominence, do they reflect as much difference as contrastive stress does?

All the theories discussed above predict that the order of noun phrases in non-contrastive dative sentences is semantically significant. Chomsky claimed that the last noun phrase is the most prominent, because it is at the intonation centre of the sentence. Chafe believed that all noun phrases except the first should be prominent because they convey new information. Lockwood indicated that the first nominal constituent in a simple dative sentence is the most prominent one.

Halliday believed that the first noun phrase is emphasized because it is the theme, while the new information also carries emphasis (1968,p.212). For non-contrastive sentences, the new information may be the entire information unit. However, stronger stress, which may not necessarily be contrastive according to Halliday, would narrow down the new information to the final noun phrase. Two predictions are possible. One is that the theme in a non-contrastive sentence, carrying both kinds of emphasis, should be more prominent than the other noun phrases. The other prediction, using the notion of strong but non-contrastive stress, is that the first and last noun phrases in a dative sentence are more prominent than the middle noun phrase.

The theories of Chomsky, Chafe and Lockwood thus lead to the prediction that passivization is a focus device. Chomsky's model further predicts that dative movement is a focus device, with the last noun phrase in the sentence being the focused one. Chafe and Lockwood, however, would consider dative movement to be of significance only if it results in changing the first noun phrase of the sentence. That is, only if dative movement occurs in conjunction with passivization does it have a focus effect in

their theories. The effect, according to Lockwood, is to emphasize the first noun phrase, while Chafe makes the opposite prediction.

The question of the relative strength of focus effects is related to the concept of marked focus. In one interpretation of this concept, the focused constituent in a sentence with marked focus should be more important than the other constituents, while in sentences with unmarked focus, this may not be true. Further, the marked focus should be more important than any constituent in a sentence of unmarked focus with the same propositional content. According to Lockwood's account, the first noun phrase in passive sentences should carry such prominence. Johnson-Laird (1968) also expressed this view.

The experiment reported here, then, was conducted in order to investigate the focus properties of passivization, dative movement and contrastive stress with reference to the above questions.

Method

A sentence family was constructed which consisted of the syntactic variants of a simple dative sentence which are obtained by selecting combinations of active or passive voice and forward or back dative position:

- (1) John gave the book to Andrew.
- (2) John gave Andrew the book.
- (3) The book was given to Andrew by John.
- (4) Andrew was given the book by John.

For each of these sentence types, five stress patterns were introduced: normal intonation, or contrastive stress on either the verb, agent, direct object or indirect object. There were therefore twenty distinct sentences.

The sentences also varied in semantic content, with similar lexical items substituted for those in sentences (1) - (4) above. This variation in semantic content was not considered to influence the subjects' judgements, and was not treated as a factor in the data analysis.

Each of the twenty sentence types was presented once to the subjects in one of the four semantic contents. The sentences were tape-recorded, in a randomized order.

The subjects were 79 English-speaking high school students attending Jasper Place Composite High School in Edmonton. After hearing a sentence on the tape, the subjects were provided with a written version of the sentence (in which contrastive stress was not indicated), and a list of the agent, verb, direct object and indirect object from the sentence. They were instructed to rank order these four words according to their importance to the

meaning of the sentence, 1 indicating the most important and 4 the least important word.

Results

Two methods of analysing the data were employed. First, the most reliable segment of the ranking process is the subjects' first rank choice for each sentence. From this set of data², it is apparent that the first noun of the sentence is regarded as more important than the other nouns or the verb. For example, when stress occurs on the first noun, it is chosen as the most important element by 81% of the subjects. In unstressed sentences, it is chosen by 65%. When stress occurs on the verb, second noun or third noun, the first noun is still chosen as most important by 31%, 39% and 43% of the subjects respectively, while the stressed element is chosen by only 58%, 44% and 34% respectively. These results suggest that there are two kinds of focus device operating in these sentences. The first is contrastive stress; the second involves word order — in particular, front-shifting seems to be a focus device.

Front-shifting is represented in the stimulus sentences by passivization and dative movement. The effects of these operations separately, as well as that of contrastive stress, were investigated using an analysis of variance. The following effects were highly significant ($p < .001$). First, contrastive stress increased the importance of the word to which it was applied. Secondly, passivization decreased the importance of the agent, while increasing prominence for the direct object and indirect object. Thirdly, dative movement, which results in the indirect object coming before the direct object in the sentence, increased the importance of the indirect object, decreased the importance of the direct object, and had no effect on the agent rankings.

The prominence of the first noun is thus demonstrated by the first-rank data results, and by the effects of passivization and dative movement. The dative movement effect also suggests that the second noun may be more prominent than the third. Therefore, it was of interest to see whether interchanging only the second and third nouns in the sentence has any effect. Therefore, an analysis of variance was carried out on the active sentences only. Dative position was found to have a highly significant effect ($p < .001$) on the subjects' rankings of the direct and indirect objects. The results indicated that the second noun in active sentences is in fact regarded as more important than the third noun.

Front-shifting and contrastive stress are identified by the experimental results as focusing devices. Is contrastive stress the stronger of the two, as the theories suggest? The first-rank data indicate that when the second or third noun is stressed, the stressed noun is chosen as most important by 39% of the subjects,

while 41% choose the first noun. Thus, the first position in a sentence seems to convey as much prominence as contrastive stress does.

A second, more general, measure of the relative importance of stress and position is the mean rank assigned to the first, second and third nouns in both the unstressed and stressed cases. These mean ranks, which were computed from the analysis of variance results, are graphed in Figure 1. This figure illustrates how much change in perceived importance is caused by stressing a noun in a given position, or by altering its position. Stressing a noun in any position brings about a significant increase in its importance. However, Figure 1 shows that contrastive stress has a smaller effect the further forward the stressed noun is located; the first nouns in a sentence are already emphasized by their position.

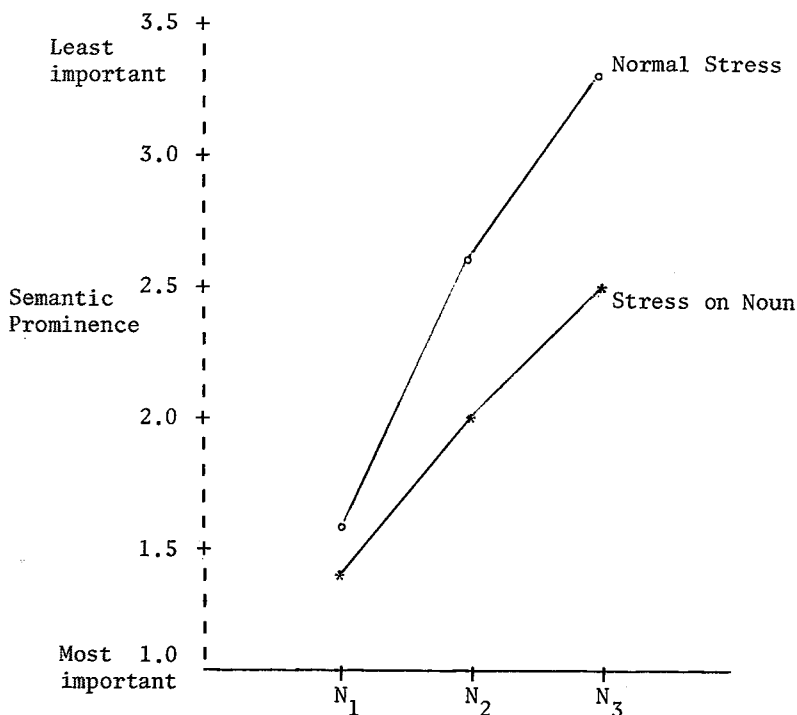


Fig. 1. Effects of stress and position on the semantic prominence of nouns.

Does the passive sentence form have a more marked distribution of importance than the corresponding active? In particular, is the first noun in the passive relatively more prominent than its counterpart in the active? The analysis of variance results indicate that for the normal stress situation, the mean rank for the grammatical subject in the active (i.e., the agent) is 1.68, while for the passive (i.e., the direct or indirect object), it is 1.57. This difference is not statistically significant. The corresponding mean rank for the two grammatical objects is 2.96 for the active and 2.94 for the passive. Since passivization does not make the grammatical subject more prominent relative to the objects, or relative to the subject in the corresponding active sentence, the passive sentence form cannot be said to be "marked" in this sense.

Discussion

The focus theories which indicate that the intonation centre conveys prominence are inaccurate in predicting the experimental results for sentences with normal intonation, although they do predict the increase in prominence brought about by contrastive stress. That is, the concept of given and new information, as developed in these theories, may be a semantic basis for the observed effect of contrastive stress. However, the "given-new" construct alone does not describe the observed prominence pattern in sentences with non-contrastive stress, since the item at the normal intonation centre is semantically the least prominent.

An interesting result concerns the focus mechanism of passivization and of dative movement. It is the front-shifted noun phrase that is prominent, not the noun phrase that is back-shifted to the intonation centre of the sentence. Another interesting point emerges from the comparison of the relative impact of contrastive stress and front-shifting as focus devices. As shown in Figure 1, the data indicate that contrastive stress is not a more powerful focus device than front-shifting; rather, the two processes simply represent different ways of focusing information.

The data, then, favor an approach, such as Halliday's, in which a word order factor operates independently of an intonation factor in reflecting the pattern of prominence of the words in a sentence. However, the results also reveal two inaccuracies in Halliday's approach. First, Halliday stated (1968, p.213) that the clause-final item, even under normal intonation, carries prominence because it represents new information. However, for sentences of the experimental type at least, non-contrastive sentences do not convey to the listener any impression that the item at the intonation centre is focused.

Secondly, Halliday's theory predicts only part of the relation, shown in Figure 1, between word order and importance. According to his theory, prominence is conveyed by word order only for the first noun phrase of the clause. This concept of

"theme" requires revision in order to accommodate the experimental results concerning dative position, which indicated that the second noun in a sentence is seen as more important than the third noun.

The effects reported here, of course, have been demonstrated only for sentences of the experimental type. Other sentence variations might alter the results. For example, longer sentences might result in subjects choosing the first and last constituents as most important. If the linear order effect does not persist, then the focusing mechanism of dative movement, for example, might not be front-shifting.

Secondly, all the agents and indirect objects in the stimulus sentences were proper nouns, while all the direct objects were definite noun phrases. Variations in the noun phrases must be investigated as part of the question of the semantic basis for focus, especially if the theories maintain an emphasis on old and new information. There is already experimental evidence that pronouns and definite noun phrases tend to express old information, while proper nouns and indefinite noun phrases indicate new information (Grieve, 1973). Syntactic correlates at the sentence level for old and new information have also been verified (Hornby, 1972). However, both the interaction of these two ways of conveying old and new information, and the focusing properties involved, require further investigation.

The results of the present experiment, which is only a step toward a thorough investigation of focus, have indicated some areas in the focus theories which need to be revised. From this finding, it is clear that a wide range of psycholinguistic experiments is a necessary contribution toward an adequate theory of focus.

Footnotes

1. Chafe (1976) claimed that a contrastively stressed constituent need not represent new information. Thus, for contrastive sentences, he has moved away from the idea that only new information is prominent.
2. The data are presented in Andrew (1974).

References

- Andrew, C. M. An experimental approach to grammatical focus.
Unpublished doctoral dissertation, University of Alberta, 1974.
- Chafe, W. L. Meaning and the structure of language. Chicago:
University of Chicago Press, 1970.
- Chafe, W. L. Givenness, contrastiveness, definiteness, subjects,
topics, and points of view. In Charles N. Li (Ed.),
Subject and topic. New York: Academic Press, 1976. Pp.25-55.
- Chomsky, N. Deep structure, surface structure, and semantic
interpretation. In D. D. Steinberg & L. A. Jakobovits (Eds.),
Semantics: an interdisciplinary reader in philosophy,
linguistics and psychology. Cambridge, England: Cambridge
University Press, 1971. Pp.183-216.
- Grieve, R. Definiteness in discourse. Language and Speech,
1973, 16, 365-372.
- Halliday, M. A. K. Notes on transitivity and theme in English.
Journal of Linguistics, 1967, 3, 199-244 and Journal of
Linguistics, 1968, 4, 179-215.
- Halliday, M. A. K. Language structure and language function. In
J. Lyons (Ed.), New horizons in linguistics. Harmondsworth:
Penguin Books, 1970. Pp. 140-165.
- Hornby, P. A. The psychological subject and predicate. Cognitive
Psychology, 3, 632-42.
- Johnson-Laird, P. N. The choice of the passive voice in the
communication task. British Journal of Psychology, 1968,
59, 7-15.
- Lockwood, D. G. Introduction to Stratificational Linguistics.
New York: Harcourt Brace Jovanovich, 1972.

THE RELATION BETWEEN LINGUISTIC PROXIMITY

AND EXTRALINGUISTIC CONTRAST

Ulf Bäcklund
University of Umeå, Sweden

Abbreviations

- ALD *The Advanced Learner's Dictionary of Current English.*
 Oxford University Press.
- BC Francis, W.N., *A Standard Sample of Present-Day American*
 English, for Use with Digital Computers. Brown University.
 (Generally referred to as 'the Brown Corpus')
- W *Webster's Third New International Dictionary.*

The principal purpose of the present paper is to show that syntagms containing lexical items with closely related semantic contents function to emphasize an extralinguistic contrast in such a way as to "push apart" the extreme poles of such a contrast. This will be done by means of an analysis of a group of words which share the semantic feature 'negation of a negation'. The group consists of the words *faint*, *stir(ring)*, *barely*, *hardly* and *scarcely*. 'Negation of a negation' implies a crossing of a dividing line between opposite ideas. The general principle is that linguistic items in co-occurrence are placed as close to this dividing line as possible, constituting a "dynamic" centre with the power to increase the distance between extralinguistic contrastive ideas. Before we discuss the words constituting the group which illustrates this general principle, we will look at two rather complicated sentences which clarify the relation between linguistic proximity and extralinguistic contrast.

- I Fundamental values, temperament and the way in which one approaches a conviction change less, of course, than specific opinions. That fact is very clearly illustrated in the case of the many present-day intellectuals who were Communists or near-Communists in their youth and are now so extremely conservative or reactionary, as many would say that they can define no important political conviction that does not seem so far from *even a centrist position as to make the distinction between Mr. Nixon and Mr. Kruschew for them hardly worth noting.* (BC)

The extralinguistic contrast focalized here is the one between "many intellectuals" and other people as regards their political viewpoints. The instability of the political opinions of "many intellectuals" is contrasted with the stability of "important political convictions" in other people. This contrast is given an effective note of irony by directing the reader's attention to the dividing line between *Communist* and *non-Communist*. The italicized part in 1 is the linguistic proximity which sheds light on "extremely conservative or reactionary". The semantic structure in 1 may be described in the following way:

- 1 The political standpoints of many intellectuals have proceeded from Communism to extreme conservatism.
- 2 The intellectuals deny this, saying they have a stable political conviction.
- 3 If this denial is valid, the intellectuals' political conviction would have the implication: "the distinction between Mr. Nixon and Mr. Kruschew is hardly worth noting".

The ironical effect in 3 is achieved by the equivalence of the degree of conservativeness and the object of *say* in "many would say". This equivalence is marked by *so* and *as*. The object of *say* represents a treatment of the intellectuals' denial in 2 with mock seriousness, i.e., the writer pretends to agree that the fickle political opinions are "an important political conviction", representing a type not heard of before. *Hardly* has the double function of being more compatible with *conviction* than with *opinion*, as *hardly* is correlated with permanent properties rather than temporary ones (see below), and of creating an equivalence between "distinction ... hardly worth noting" and "even a centrist position". This is accomplished by the much stronger attitudinal negative element in *hardly* than in the neutral *not*. Thus, the negation of *worth noting* is strongly emphasized by the addition of an element denoting the highest possible degree, e.g. 'absolutely not'. Accordingly, this attitudinal emphasis of the negation causes a high degree of linguistic proximity, even complete equivalence, as we said above, between "even a centrist position" and "distinction ... hardly worth noting". The adverb *even* has a crucial importance for our topic in that it marks the dividing line between 'not' and 'not not', thereby having a neutralizing effect. In our sentence this neutralizing effect has two consequences: (a) it invalidates 2 in that "a centrist position" invalidates the word *conviction* in its political sense; (b) (a consequence of (a)) it emphasizes *extremely* (*conservative* and *reactionary*), thereby increasing the distance between the contrastive poles, *Communist* and *reactionary*. Thus, *linguistic proximity enhances extralinguistic contrast*.

- 2 Our *endeavor* to capture even a faint sense of how strenuous was the fight was muffled by our indifference to the very issue which in the Boston of 1848 seemed to be the central hope of its Christian survival, that of the literal, factual historicity of the miracles as represented by the Four Gospels. (BC)

The contrast between 'make exertion' and 'impossibility to achieve anything by exertion' is enhanced by the collocation *faint sense*, whose constituents are two words in linguistic proximity. This proximity is marked by *even*. The adjective *faint* has three important semantic characteristics: (1) *dynamic*, i.e. signifying a movement. This is manifested in co-occurrence with dynamic verbs as in the following examples from BC: "the rocket became a faint speck"/"dust it down to a faint image"/"stained them a very faint green"; (2) associated with *sensation* or *perception*; (3) expressing a *state*. Its position as an attribute in front of nouns denoting objects of perception does not represent a different sense from its sense in "feel faint". Rather, a *faint speck*, for example, should be interpreted "register faintly/be faint in perceiving". *Faint* and *sense* are compatible in their designation of an incipient mental operation. *Faint* refers to perception and *sense* to conception. These two mental processes merge, and there is no sharp boundary between them. *Faint* as well as *sense* express the crossing of the dividing line between *not conscious* and *conscious*. *Sense*, however, allows a heightening of degree to, for example, *strong sense*. *Even* and *faint*, which are obligatory,¹ arrest this upward movement of *sense* at a point as close to the dividing line as possible. The lexical cohesion represented by the three items in linguistic proximity is extended to the predicate form, *muffled*. This verb demands a sensuous object, for example *sound* and *voice*, and a sentence such as * "His endeavor to write a book was muffled by ..." is unacceptable. The dynamic centre of the three items in close semantic proximity attaches an emphaser to *indifference* and *endeavor*. *Endeavor*, which is "stronger" than *attempt* and *effort*, represents a lexicalization of the contrast emphasis, and constitutes in addition a further extension of lexical cohesion through its semantic feature 'continuity' (cf. the *state*-quality of *faint*). W defines this word in its verbal function like this: "to attempt earnestly to attain an end by a continued series of acts, by repeated effort, or continued search for expedients". The three items constituting the dynamic centre of linguistic proximity make up a scale as to their gradability, from *sense* (for example, *strong* can modify *sense*) via *faint*, which can occur in the superlative (see below), to the non-gradable adverb *even*. This is a neutralizer, which "pulls" down *faint sense* to the lowest point imaginable of the gradable idea which is the opposite of 'not conscious'. Because of the emphatic nature of this opposite, 'Not notconscious' is a more appropriate designation of this idea than 'conscious'.

Consider the following sentences:

- 3 The *faint beginnings* of fear *stirred* in his mind. (BC)
- 4 A *faint smile* *stirred* her lips. (W)
- 5 The evening was pleasantly cool, with *the faintest stirring* of a light breeze. (Hammond-Innes).

We will claim that the verb *stir* shares the semantic feature *dynamic* with *faint* in that it implies a movement, or rather, the *result* of the movement from *non-existence* to *existence*. Put somewhat differently, the main function of *stir* is to signify the crossing of the dividing line between 'not' and 'Not not', i.e. *stir* corresponds with the emphatic 'NOT'. The emphasis of this element is derived from the conspicuousness of an incipient perception. A corollary of this function of *stir* is that it needs support from the adjective *faint* in sentences which are marked by incipient perception, i.e., sentences which convey new information. *Faint* is the natural support in such sentences because this adjective denotes the resulting state of a movement coming from the + side of the dividing line, so to speak. Thus, *faint* moves down to its lowest point to meet *stir* and give it the necessary support. This point marks the minimum condition for a perceptual process to take place. We can note here that the verb *faint* is intimately related with the adjective *faint* in that it denotes a movement away from the lowest point of the adjective. In fact, the meaning of the verb, 'lose consciousness', is an indication of the complete compatibility that prevails between *faint* and *stir* in its denoting the crossing of the dividing line between *consciousness* and *non-consciousness*. Consequently, the verb *faint* denotes a movement from + to-, whereas *stir* denotes a movement from - to +.

Sentence 3 is much more effective than "he could notice the beginnings of fear in his mind" because of the dynamic power of the three items in linguistic proximity to imbue *fear* with a strong emotional significance. Sentence 4 probably occurs in a situation where a certain person is not very likely to show any reaction at all. Furthermore, the situational context is likely to suggest that *if* a reaction should appear, it would have the characteristics of a *smile*. Thus, *smile* and *fear* constitute interpretations, on the basis of the situational context, of an incipient perceptual process which cannot by itself form the basis of such an interpretation. This accounts for the redundancy in "smile stirred her lips". *Smile* is an interpretation of the indeterminate *something*, which is the object of *faint* and the subject of *stirred* in the underlying semantic structure. This can be described like this:

I can faintly register something which stirs her lips.
I interpret something as being a *smile*.

This underlying structure reflects the opposite directions of the

movements expressed by the verbs *stir* and *faint*, as described above. The definite article in 3, "*the faint beginnings ...*", indicates that *stir* has a generic nature, because the 'givenness' expressed by the article is here related with common knowledge with the suggestion: "you know" what I mean. This 'givenness' is also manifest in 5. The superlative form of *faint* implies a duplication of the marking of the lowest point of perceptibility, which gives additional emphasis to the marking of 'NOT' in 'NOT not move'. It is significant for our theme that this duplication occurs in the head of *with*. There is a suggestion of complete *identity* between the verb/adjective preceding *with* and its head, for example, "her eyes blazed with anger": "blazing anger", where *blaze* is a metaphorical item for *anger*: "there was anger, a *blaze*, in her eyes". The time preceding the "evening" in 5 is likely to have been unpleasantly hot with *absolutely no wind*. The crossing of the dividing line between 'no wind' and 'NOT no wind' has a specific significance here because 'the negation of a negation' is looked on as the cause of the contrast of 'unpleasantly hot'. As a consequence, the linguistic proximity requires extra emphasis.

The generic nature of *stir* derives from it denoting an incipient perceptual process. As we said above, incipience of perception is linked with conspicuousness. Hence, the negative element which is superimposed on another negative element is given emphasis. Thus, we can define the meaning of *stir* 'NOT not move'. The verb *move* in this definition has a generic sense, however, i.e., it is separated from a specific Agent and is thus the underlying verb of the generic noun *motion* rather than of *movement*, which is linked with a specific Agent. There seems to be a correlation between the genericness of *stir* and its significance for the context in which it occurs.

In the literal sense of *stir*, its contextual significance is manifested in lexical items which signify a low point in the scale of consciousness, as in:

6 the girl *stirred*, *moaned* and sat up. (W)

7 where Walitzee *stirred*, *mumbling*, *sitting up in a half stupor...* (BC)

What is more to the point for our theme is the tendency of *stir* to occur in negated contexts. The primary function of *stir* here seems to consist of giving as much "scope" as is imaginable to an adjectival concept. This concept may be explicit or implicit. The following sentence sheds light on the difference between a specific interpretation of 'NOT not move' and the generic one, which we claim to be the definition of *stir*:

- 8 He hit him once on the sharp point of his chin and felt the body go limp. He dropped him into the street a couple of feet away from Geely's recumbent figure and stared down at both of them for a moment before kicking the big man lightly in the side. He *didn't stir*. (BC)

Go limp implies 'become motionless'. The kicking is a test as to the validity of *limp*, i.e., the expected response is 'not move'. If the result is an *unexpected* response, the unexpectedness is represented by a specific superimposed 'NOT'. This specific emphatic 'NOT' tends to proceed indefinitely upwards, thus representing any degree of 'not move', accompanied by a corresponding degree of unexpectedness. The unexpectedness may, for example, consist of the "limp body" jumping up and starting to fight again. The generic superimposed 'NOT', on the other hand, constitutes the marker of the crossing of the dividing line between *existence* and *non-existence* of 'signs of movement', and the additional negation ('not NOT not move') implies failure to mark this crossing. The generic 'NOT not move' serves to "pull down" the adjectival concept *limp* to the point 'unconscious', giving this concept as wide a scope as is imaginable. *Not stir* may be said to function as the APEX of *limp* (see Bäcklund: 1976). The apex can be marked by *even*: so limp that he did not move, not *even stir*.

It was argued in my article "Apex Adjectives" that the apex-function is related with a concretization of an abstract concept. Such a concretization is evident in the following examples:

- 9 Not a leaf was *stirring*. (ALD)

- 10 Nobody was *stirring* in the house. (W)

These sentences serve to push apart the extralinguistic ideas 'normalcy' and 'non-normalcy'. This is achieved by *stir* giving the adjectival concepts 'windless' and 'quiet', which underlie "there was no movement in the trees/in the house", as wide a scope as is imaginable. The entity concepts "tree" and "all the people" are particularized, yielding forms which are closely associated with *motion*,² i.e., "even if there is no movement in the trees/in the house, a single leaf/body (belonging to a person alive) is never motionless". Both subject and predicate are here apex forms, as the following paraphrase, which may be said to be the underlying semantic structure, suggests:

not even a single leaf/body even reached 'NOT not move'.

Noone is unlikely to replace *nobody*, because *nobody* is more generic (see Bolinger: 1975).

There is implicitly a similar particularization in the following two sentences:

11 I haven't *stirred* out all morning. (W)

12 it was very wet all day, and I didn't *stir* out of the house. (W)

The implication is: *normally* I go out at least once during the time expressed by "all morning/day", but for this specific morning/day the following statement applies: not even on one single occasion did I even reach 'NOT not move'. This highlighting of 'non-normalcy' serves to attach semantic focus on *wet* in 12. A similar focalized concept constituting an explanation for the 'non-normalcy' is likely to occur in the context of 11.

In those cases where *stir* is a presupposition for the concept which is given an extended scope, *stir* seems to require a linguistic item which functions as a marker of the level 'NOT' in 'NOT not move'. Consider for example:

13 talk freely on *everything* that *stirs*. (W)

14 a bed that squeaks, if he *so much as stirs*. (W)

15 little boats that *barely stir* its mysterious waters.
(W)

In 13 and 14, the properties of *talk* and *bed* are pushed up to their apex points. *Everything* is completely compatible with *stir*, because this subject is a corollary of the reaching of the upper limit 'NOT', expressed by *stir*: if something 'NOT not moves (exists)', he talks about it, i.e., he talks about *everything*. As *freely* is an absolute concept, *stir* may be said to ensure the genuineness of this feature. *Squeakiness* of beds is dependent on someone moving when lying on them. The suppression of 'move' to its lowest imaginable point by means of *stir* requires a degree modifier marking this point. Thus, linguistic proximity is necessary to express the apex of *squeak*. Similarly in 15, *barely* is necessary to signalize the lowest point of 'mark made by boats on water', and the collocation *barely stir* forms a dynamic centre pushing apart the contrastive adjectives inherent in the two Noun Phrases which function as the subject and object of *stir*: *small* boats, *vast* expanses of water.

The collocations *hardly/scarcely stir* in the following two sentences serve to focalize the properties *tied tightly* and *intent*:

16 tied so tightly that he could *scarcely stir* a finger.
(W)

17 so intent on this fantastic ... narrative that she
had *hardly stirred*. (W)

Barely, as we have seen, may be said to have the feature 'negation of a negation', which we said in the introduction was the common semantic trait of the group of words discussed. *Barely* satisfies this criterion by marking the emphatic element 'negating a negation', which is a constituent of the dynamic verb *stir*. *Scarcely* and *hardly*, on the other hand, attach an additional negative element. Thus, these two adverbs mean 'negation of a negation' in collocation with *stir*. Both *hardly* and *scarcely* are linked with an "as if" element. In the case of *hardly*, this element is attached to the head of the focalized property, thus suggesting a *permanent* quality, whereas *scarcely* attaches this element to the topmost negative element, "as if not stir a finger". A consequence of this is that *scarcely* focalizes *symptoms* of a property which has a *temporary* character. Before we return to 16 and 17, we will discuss the "synonymous" group which these three adverbs constitute.

Barely, *hardly*, and *scarcely* are synonymous in that they may be said to mean 'almost not'. However, if the varying degrees of an attitudinal element being attached to these "synonymous" adverbs are taken into account, their semantic function differs widely. There is a rising scale in this respect from *barely*, which is non-attitudinal and factual, i.e. non-metaphorical, via *scarcely* to *hardly*. The difference between *barely* and *hardly* as to their association with an attitude and their metaphoricalness causes them to function as *antonyms* rather than *synonyms* in collocation with *escape* in 18 and 19:

18 He *barely escaped* being shot.

19 And the election of President Kennedy has attracted new attention to the ethical climate of this home state. A reader of the Boston newspapers can *hardly escape* the impression that petty chicanery, or worse, is the norm in Massachusetts public life. (BC)

Barely functions solely to stress the validity of the concept expressed by *escape*. This concept is given strong extralinguistic significance by the fact that *barely* signifies, as it were, the shortest possible distance between the non-negated form and the negated form, *not escape*. The extralinguistic significance which *barely* gives to its head is due to *barely* attaching an emphatic 'NOT' to the negated head, 'NOT not escape'. Following this, the collocation *barely stir* should be given the "reading" 'NOT1 not1 stir (*barely*) NOT2 not2 move (*stir*)'. As we have argued above, 'not1' implies 'not reach NOT2' because of the dynamic nature of *stir*. Hence, the function of the emphatic 'NOT1' is to erase 'not' in 'not reach NOT2'. This would explain the obligatoriness of a degree marker in 14 and 15. *Barely* and *so much as* so to speak pull up *stir* from its common negative environment. The collocation *hardly escape* is rather more complex because *hardly* is involved in a contextual network. *Hardly* functions to stress the validity of the *negation* of the concept expressed by *escape*. Hence, *escape*

gets the value 'absolutely not'. This meaning is *argumentative*, not *factual*, as in *barely escape*. This means that the speaker wants to reject a real or, as is most often the case with *hardly*, a pretended contradictory statement: "some people may say that *escape* is valid". Thus, *almost* in the definition of this group of adverbs, 'almost not', here stands for a movement away from this contradictory statement. The attitudinal element in *hardly* causes this movement to proceed indefinitely. This has two consequences: (1) the object of *escape* must be abstract; (2) (linked with (1)) the object of *escape* contains what I choose to call "linguistic creativity". This "linguistic creativity" marks a pragmatic end-point of the indefinite movement expressed by 'almost', thus justifying the interpretation 'absolutely not' rather than 'almost not'. The marking of this pragmatic end-point serves to back up an assertion occurring in the preceding context. In 19, this assertion has been given the linguistic manifestation "has attracted new attention to the ethical climate ...". The context implies that this means attention to criminal acts. The piece of "linguistic creativity", "petty chicanery, or worse, is the norm", is a consciously exaggerated concretization of the assertion made in the previous context. We may call this concretization, which marks a pragmatic end-point of the indefinite movement provoked by means of *hardly*, the APEX of this assertion.³

We will claim that there is a similarity between *faint* and *scarcely* in that both have a metaphorical function with respect to something *natural* or *observable*, whereas *hardly* is, as we have seen, metaphorical in respect to something *asserted*. A further example with *hardly* will clarify this:

- 20 ... and drove home a solid left to Jess's mid-section. It was like hitting a sack of salt. Pain shot up Curt's arm clear to the shoulder, but Jess seemed *hardly aware that he had been hit*. (BC)

The italicized part is the concretized apex of the assertion "like a sack of salt" and is here a necessary consequence of this assertion. Thus, the apex serves to underscore the validity of *like*. The non-humanness which the apex expresses lies in the incompatibility, or the "unnaturalness", of the combination of *aware* and its modifier (the *that*-clause). The semantic structure is: not aware, not *even* of being hit, thus he is *really* like a sack of salt. The assertion is 'given' (I), "not aware" may be termed symptom (II), and "that he had been hit" (III) is the apex of this symptom. We get the schema:

I	II	III
<i>as if</i> a sack of salt	not aware	that he had been hit

The adjective *faint*⁴ occurs in the example "the faint atmosphere of a tropical port" in W. As there are several examples of a "transference" of adjectives from in front of the preposition *in* to the head of this preposition in cases where there is a close semantic link between the adjective and the noun, e.g. *profuse in his apologies: profuse apologies/untiring in his efforts: untiring efforts*, we can posit the underlying structure "faint in the atmosphere of the tropical port". Now, *tropical port* is 'typical' or 'given' in the sense that it has the "naturalness" of provoking symptoms like "dizziness". As we have seen, *faint* is an apex term in its marking the lowest point of 'consciousness'. Thus, *faint*, on the point of losing consciousness', functions as an expressive "stand-in" for the symptom "dizziness", which is present in the underlying structure. Accordingly, the "as if" element is placed under III rather than under I:

I	II	III
tropical port	dizzy	as if faint

The informants consulted assure me that *hardly* is not likely in the following examples:

- 21 Marian fled down the stairs, her feet *scarcely touching* the floor, and ran along the corridor. (Murdoch)
- 22 Denis seemed to be walking lightly over the surface of the bog, his feet *scarcely touching* the ground. (Murdoch)
- 23 Thompson himself *scarcely glanced* at the treasures that he was seeing together for the last time. In fact, he seemed delighted to get rid of them. (BC)
- 24 Among them were two baby donkeys *scarcely bigger* than fox terriers. (Murdoch)
- 25 But he could *scarcely yet* bear to think of the recent past as real. (Murdoch)

The main semantic function of *scarcely* is most clearly demonstrated in 25: to "bear to think" of something one *knows* has happened "as real" is inescapable. *Scarcely yet* expresses the psychological mechanism of repression by penetrating the boundary between what is an incontrovertible fact and the impossible "otherness". *Yet* collocates with *another* with the connotation of 'on the other side of what is normal or expected', as in "yet another side of his character" (W), and "by ordinary post, as I could furnish yet another, if that went wrong" (W). Consider also the following sentence where *yet + head* underscores the negation of *vote* by denying the presupposition for this act to take place:

26 have never voted for him, *nor yet intend to*. (W)

In the other sentences, *scarcely* + *head* has an apex function by giving a feature of an act or a noun ("*baby* donkeys" in 24) as wide a scope as imaginable. *Seem* in 22 and 23 indicates that "walk lightly", "fled", are inferential lexicalizations based on the perceptual *appearance* 'walk in a way as if feet are not touching ground/floor'. In 23, the inference is extracted from 'see treasures in a way as if not looking at them' in the form of an adjective occurring to the "right" of *scarcely* (this is important for the difference between *scarcely* and *hardly*, as we shall see). In 24, the modifier *baby* seems to be an inference from 'appeared not bigger than fox terriers' in that the following might be added: hence, I call them *baby donkeys*. *Scarcely* faces "otherness" in respect to what is natural: the way of locomotion I perceive implies 'feet touch ground/floor', but it has the feature 'as if not touch'; donkeys have a standard size, but the size of the donkeys I perceive faces the size of "fox terriers". The "linguistic creativity" represented by "fox terriers" is radically different from the one characteristic of *hardly*. "Fox terriers" is taken from a field placed next to the "natural" field whereas the "linguistic creativity" associated with *hardly* implies a deliberate search for a field as far away from the "natural" field as possible.

In 21, *scarcely touching* justifies the lexicalization *fled*, ruling out more moderate terms for this kind of locomotion in the following way: appears not to touch the floor, hence she *fled*, much less did she touch the floor in a way that justifies the term *walk*. Now, *tied tightly* has the purpose "prevent somebody from acting by making him unable to move". (As if) *not stir a finger* faces "otherness" in that it marks utter impossibility of 'acting': not even 'NOT not move' even a finger, much less could he 'move to act'. Thus, *scarcely stir a finger* justifies the term *tied tightly* in much the same way as *scarcely touching* justifies the term *fled*. An inference can be made from the perception 'appeared not to stir a finger' to 'he seemed to be tied tightly'. The emphasis lies on the property as such. In the case of *hardly*, however, a *claim* is made as to the state of the *head* of the property (cf. sentence 20). From this argumentative claim the inference is made that the property may be CALLED *intent*. Consequently, *hardly* and *scarcely* are each other's opposites, as is evident in this schema:

I	II	III
tied tightly	not move to act	as if not stir a finger
as if dead	not stir	intent

Scarcely stir a finger emphasizes an *observable* property, whereas *hardly stir* emphasizes a *non-observable* property. Both collocations are "dynamic centres" serving as reflectors of *not (tied)*

tightly and not intent.

We will now examine a number of sentences with *hardly* and *scarcely* to see if the difference between them, as it appears in the above schema, is due to a fundamental semantic distinction between them.

TABLE

EXAMPLES ILLUSTRATING THE DIFFERENCE BETWEEN "HARDLY" AND "SCARCELY" (from BC, if not stated otherwise)

- 1 ... no actress can be expected to do her best work when her fortune, her reputation, her livelihood, her home and her nation itself are all imperilled. Such sweeping distractions are *hardly conducive to Oscar winning performances*.
- 2 ... the many unglamorous winter months when your pool will *hardly look its best*.
- 3 This is *hardly the time* to discuss matters. (W)
- 4 It is *hardly possible to emphasize this too much*.
- 5 It's *just barely possible* with this crowd that the kidnapper wasn't able to get a table.
- 6 He *could hardly believe* he was getting in (into a car).
- 7 It was *hardly credible* --- but there he was. (Informant)
- 8 She *could scarcely believe it*, as she dressed herself, trembling and shivering already with excitement, that she, today, was to be the one to break the spell which had dazed and defeated so many. (Murdoch)
- 9 It was *scarcely credible* that she should have done this. (Informant)
- 10 The gulf between the rich and the poor has narrowed, in the industrialized Western world, to the point that the word "poor" is *hardly applicable*.
- 11 The conversation that ensued may have been engrossing but it could *hardly be called world-shattering*.
- 12 It would *hardly be an exaggeration to say...*
- 13 I felt strongly attached to the hall, however, and *hardly a day passed when I did not go to look at it from a distance*.

- 14 And no wonder, for Vientiane, the old City of Sandalwood, had become the City of Bullet Holes ... Along the main thoroughfares *hardly* a house had not been peppered.
- 15 There was something about the contour of her face, the smile ..., the way she held her head, the way she walked ... there was *scarcely* anything she did which did not fascinate me.
- 16 No other names among the young men in residence at the time seem to have been even suggested by Milton as those of persons with whom he in any way consorted. But that *scarcely* means that he was the aloof, forbidding type of student ..., the banter of the surviving prologues being enough evidence to deny this.
- 17 He showed me one of his fingers which had been practically amputated and which his mother had treated; there was *scarcely* a scar showing.
- 18 ... I could not have felt more fear. *With scarcely* a mumble of excuse, I fled.
- 19 I saw *scarcely* a body, except a black-coated man.
(Rebecca West)
- 20 *Scarcely* any golf of any importance was played at all.
(The Sunday Times)
- 21 ... so small as to be *scarcely* visible to the eye.
- 22 They all had the hard look of gamblers who had stopped dreaming, who automatically turned the cards, *hardly* caring what showed up.
- 23 *Seemed scarcely to notice* what passed ... as if he were in some partial coma. (W)
- 24 He was only vaguely aware of the sluicing rain. He *hardly* noticed the blue-green flashes of lightning and the hard claps of thunder.
- 25 He *appeared scarcely aware* of the presence of Mrs. Whitehead. (The Observer)
- 26 ... while the other kids laughed and yelled in at him and made faces as they dispersed, going home. He *scarcely* saw them. His heart was pounding like a mighty dynamo and he was trying to think, his mind seeming to scream at him like a hurt or frightened child, "How will I do it?"

- 27 [*hardly able*] to breathe, beginning to choke. (stage direction) (David Pinner)
- 28 On such a day, he passed through the close from the deanery to the west door, *hardly able* to see his feet for the fog. (William Golding)
- 29 *Hardly less important* than any historical document is its history. (The Sunday Times)
- 30 ... but to vote unanimously that the United Nations was *scarcely less than wonderful*, despite an imperfection here and there. It was, the brief writers decided, "man's best hope for a peaceful and law abiding world". Peace, it's wonderful, and "world law", it's wonderful, too ...

The capacity of *hardly* to emphasize a non-observable concept is linked with a higher degree of what can be called "encoding depth" than in the case of *scarcely*. This is also connected with the stronger attitudinal element in *hardly*.

Basically, *scarcely* questions the validity of its head, its context indicating the reasons for the justification of such a doubt. *Hardly*, on the other hand, is more dependent on the speaker's pragmatic knowledge of the world. This can be illustrated by means of 27, 28, 29, and 30. If we replace *hardly* by *of course*, we get an acceptable collocation in 27, "of course, able to breathe", and an unacceptable collocation in 29, "of course, less important ...". The acceptability and unacceptability are related with an apex-character: if there is anything a person is able to do, it is breathing/if there is anything which may be said to be absolutely less important than history, it is the documents based on history. *Hardly* gets its strong contextual significance in referring to the state of the Agent in 27 and to the denseness of the fog in 28 through the triviality of the combination of *able* with *breathe* and *see one's feet*. The same triviality is used in 29 to achieve the strong ironical effect. The encoding process may be described in the following way: it's stupid to make the comment "more important", even more stupid to say "not more important", and to say "less important" is the peak, or apex, of stupidity. By preposing *hardly* to *less important*, the speaker pretends that somebody has *really* made the "apex"-statement. In 30, however, the speaker interprets what he can observe. The encoding process is here: from what I can perceive, the people "voting" appear to make the judgement *less than wonderful*. The attitudinal element of *scarcely* consists of the speaker's denunciation of this interpreted attitude: of course it's less than wonderful, but they appear not to think so.

There is not space for more than a sketchy analysis of the difference between *hardly* and *scarcely* as it appears in the illustrative examples. Principally, the greater "encoding depth" of *hardly* implies an *extraction* of a linguistic element or a *delayed* selection of the head. The difference between *hardly* and *scarcely* is manifested in:

1 HARDLY: *a clash with reality* in 6 and 7, where the simultaneity between the act and the doubt as to the reality of this act illustrates the contrastiveness of *hardly*. The argumentative 3 is similar: someone is going "to discuss matters". The speaker extracts "the time" in his refusal to respond.

SCARCELY: The same collocations in 8 and 9 are accompanied by a hint at the possibility of scepticism and, in 8, contextual reference to the state of the person explaining the disbelief.

2 HARDLY: *"linguistic creativity"*

- a) evoking apex-expressions: 1, 2.
- b) extraction of comments having *"call"*-nature: 4, 10, 11, 12.
- c) exaggerated particularization of entity: 13, 14 (note also *peppered*)

3 HARDLY: *referring to the head of property:*

- a) items summarized in a "name" ("sweeping distinctions"): 1 (cf. 4 a)).
- b) feelings adjectivalized to the *left* of *hardly*: 13 (cf. 4 a)).
- c) 'typicality' of "name" occurring to the *left* of *hardly*: 14 (cf. 4 b)), 22
- d) the state of the head is focalized by the contrastiveness of *notice* and head of *notice*: 24. Cf. *"hardly aware* that he had been hit" (sentence 20 in the text).

4 SCARCELY: *emphasis on property itself:*

- a) items summarized in Verbal Phrase to the *right* of *scarcely*: 15.
- b) 'typicality' of "name" occurring to the *right* of *scarcely*: 16

5 SCARCELY: *Focussing on the justifiability of its head:*

- a) giving wide scope to concept in context 17: (mother's *good treatment*), 19 (*black-coated man* is focalized).
- b) occurring after *seem* when there is an indication of the state of the person explaining "as if not": 23.
- c) occurring after *appear* when there is no such indication: 25.
- d) constituting a "compromise" between two contradictory sets of circumstances: 16, 26.

6 SCARCELY: *Similarity with FAINT:*

- a) occurring with a form indicating reduced degree (cf. *faint speck*) 18 (cf. *faintest stirring* after *with* in sentence 5 in the text)
- b) repetitiveness or redundancy: 18, 20, 21.

7 *Similarity with STIR*: (cf. sentence 10 in the text)

SCARCELY: 19. *SCARCELY* seems to replace *ALMOST* + the prefix *NO-*.

8 The head *in itself* being redundant (cf. *scarcely visible to THE EYE* (21)) because it is an inference from an assertion concerning the head of property (this point is closely linked with point 3):

HARDLY: 22 (*caring* is an inference from *AUTOMATICALLY turned up*)

Let me add three brief comments to this survey: (1) The lower degree of "encoding depth" in *scarcely* is manifested most clearly in the fact that lexicalizations of head + properties tend to occur to the "right" in the case of *scarcely* and to the "left" in the case of *hardly* (see points 3 and 4). Another circumstance supporting this claim is the similarity of *scarcely* to *faint* and *stir* (see points 6 and 7). This is an indication of *scarcely* being associated with perception. I wish to call attention to two points which are particularly interesting. The claim that *scarcely* faces "otherness" tallies with the dynamic nature of *faint* and *stir*. The obligatoriness of the adjective *faint* in sentences marked by incipient perception (3, 4, and 5 in the text) and the obligatoriness of a degree marker or a generic noun as a redundant element in collocation with *stir*, giving the emphatic 'NOT' in 'NOT not move' its necessary support, has a parallel in redundancy in the contexts of *scarcely*, e.g. "visible to the eye" (sentence 21 in TABLE)⁵. Cf. also *mumble* in sentence 18 in TABLE and *mumbling* in the context of *stir* in sentence 7 in the text. Another interesting point is the collocation with the generic noun *body* in sentence 19 in TABLE, where *scarcely* seems to replace *almost* + the prefix *no-* with the function to focalize "black-coated man" (see points 5a and 7). This focalizing effect may be explained as a coupling of the element "of course" denying "facing otherness" in *scarcely* with the emphatic 'NOT' in 'NOT not move' in *stir* (cf. the collocation "nobody was stirring" in sentence 10 in the text). Thus, the semantic structure would be: nobody (not a body) stirring, but of course someone ("black-coated man") 'NOT not moved'.⁵

(2) Sentence 10 (2 b) is particularly relevant to the function of *hardly stirred* emphasizing *intent*. The word *poor* is part of the Noun Phrase which is focalized by an "extracted" comment on the "applicability" of this very word. Similarly, we might say that the applicability of the word *intent*, which expresses a non-observable concept, but which is "known" to contain the feature [-Move], is focalized by *hardly stirred*. Thus, *intent* represents a deeper layer of the lexical store than *tied tightly*. This is in keeping with our contention that *hardly* is associated with a higher degree of "encoding depth" than *scarcely*. (3) The scale as to the degree of contextual involvement and expression of an attitude from *hardly* to *barely* has a parallel in a scale of "strength" in collocations like "can...walk" and "... any money" from *barely* to *hardly*. Thus, *hardly/scarcely/barely any money* constitutes a rising scale of desperateness of the financial situation. This is of course related with *barely* focussing solely on its head, whereas *hardly*

refers to the "left", i.e. to the Agent. "I can barely walk" implies "walk only with the greatest difficulty", whereas "I can hardly walk" is a comment on a reference in the preceding context to the state of the subject with the subaudition: look at me, I am so *X* that you can picture me as not even being able to walk. This is further support for *hardly* being closely associated with the apex-function of illustrating an abstract concept by means of a concrete image. This difference is mirrored in sentences 4 and 5. *Barely* functions as a marker of the justifiability of the use of the term *possible*, whereas *hardly* is a strong rejection of the same term.

CONCLUDING REMARKS

This paper has a bearing on my article "Apex Adjectives" in that the adjectives which were referred to this type are closely related with the apex-function represented by the "linguistic creativity" occurring in the context of *hardly*. A corollary of this is that such adjectives are not fossils, having little, if any, relation with the psychological mechanism which is responsible for speech production. Rather, they are examples of a linguistic device which is highly productive. My ongoing research on restrictive adjective-noun collocations has shown that nonceforms representing this type of adjectives are common, particularly in Noun Phrases functioning as heads of the prepositions *in* and *with*. The following example is highly relevant for the theme of this paper:

It was cold. The smoke rose straight *in an unswaying column*.
(Hammond Innes)

The four words modifying *smoke*: *rose*, *straight*, *unswaying*, and *column*, imbue, or "invest", to use Peter Maher's term, *cold* with specific significance by the fact that they evince linguistic proximity. To apply the discussion in this article, we may say that the distance from *cold* to *not cold* is increased.

The analysis of the adverbs *barely*, *hardly* and *scarcely*, which are "synonymous" in that they can all be assigned the meaning 'almost not' shows that they differ widely in their contextual functions. It is likely that words usually referred to synonymous groups tend to get more independent profiles as the amount of data serving as basis for the analysis increases. This is in accordance with Charles Ruhl's arguments against what he calls the "Compositional Assumption". The analysis of *faint* in this article also tallies with his arguments against polysemic interpretations of lexical items.

Professor H.A. Gleason remarked in one of the discussions at the IIIrd LACUS FORUM that "words don't *have* meaning, they *evoke* meaning". It may be said that *tied tightly* "evokes" the image of "somebody who can scarcely stir a finger" and that *intent* "evokes" the image of somebody adopting a posture as if he were lifeless. At the same time, *scarcely stir* and *hardly stir* affect the interpretations of these properties. Thus, it seems that the process of evoking meaning is an interaction of words in context. Furthermore, it seems that this process evinces certain regularities. The investigation of these will probably contribute to shedding light on the psychological mechanism behind speech activity.

FOOTNOTES

- 1 It may be said that *even* and *faint* are mutually obligatory, i.e., if either is present, the other one must be present as well. It seems to me, however, that "capture a sense" is only barely acceptable. The dividing line is here a sort of insurmountable obstacle for the movement expressed by "endeavor to capture" (the selection of *endeavor* rather than *effort* emphasizes this movement). This movement seems to require a similar movement from the opposite direction, manifested by *even* and *faint*. These two interdependent items "pull down" *sense* to a point where it "touches" the dividing line. The linguistic proximity that is evinced by *even a faint sense* has the effect of increasing the distance between 'indifference' and its opposite idea 'non-indifference'.
- 2 The genericness of *motion* compared with the specificity of *movement* implies a separation of the Act from the Agent. This is mirrored in *motion* permitting collocations with defining adjectives, like *circular*, *longitudinal*, *parallel*, *rotary* and *movement* permitting adjectives which refer to the state of the Agent, like *angry*, *nervous*. One manifestation of the genericness of *stir* is its collocation with noun phrases expressing totality, like *everything*, *nobody*.
- 3 The APEX discussed in 8 may be called *logical* APEX, and the one in 19 may be called *creative* APEX. The logical APEX increases the distance between an explicit form and its negated form: 'limp': 'not limp'. The creative APEX increases the difference between belief and assertion. In 19, the creative APEX, which is an elaboration of an earlier assertion ("attracted new attention...), thus causing linguistic proximity, is used to persuade those who do not believe this assertion. Thus, the APEX is here *argumentative*. The APEX adjectives discussed in Bäcklund 1976 are creative. The choice of *crashing* rather than *dreadful* in: "I had looked forward to seeing Mr. X, but he turned out to be a *crashing* bore", has a similar explanation: the gap between expectations and discovery of fact is so wide that a collocation of two words in linguistic proximity is selected.
- 4 The sense "likely to make one feel faint: OPPRESSIVE", given by W, tallies with our analysis of *faint* above, "be faint in atmosphere", "be faint in registering".
- 5 The inherent dichotomy of *scarcely* represented by the semantic constituents 'facing otherness' and the denial of this, 'of course otherness not true', has the implication that *scarcely* denotes two crossings of a dividing line. Thus, *scarcely* forms a "dynamic centre" by itself with the power to give extra emphasis to elements in the context.

BIBLIOGRAPHY

- Bäcklund, Ulf (1975) "Frozen Adjective Noun Collocations," in *The Second LACUS FORUM 1975*, Hornbeam Press, S.C.
- Bäcklund, Ulf (1976), "Apex Adjectives," in *The Third LACUS FORUM 1976*, Hornbeam Press, S.C.
- Bolinger, Dwight (1975), "The In-Group: *One* and its Compounds," in *The Second LACUS FORUM 1976*, Hornbeam Press, S.C.
- Maher, Peter (1975) "The situational motivation of syntax and the syntactic motivation of polysemy and semantic change: Spanish-Italian *bravo* etc", in Mario Saltarelli and Dieter Wanner eds., *Studies in Diachronic Romance Linguistics*, pp. 131-151.
- Maher, Peter (1976) "The Semantics and Perception of IC Structure: A Gestalt Approach to Color Terms: Spanish and English 'Green':", in *The Third LACUS FORUM 1976*, Hornbeam Press, S.C.
- Ruhl, Charles (1975) "Pragmatic Metonymy," in *The Second LACUS FORUM 1975*, Hornbeam Press, S.C.
- Ruhl, Charles (1976) "Idioms and Data," in *The Third LACUS FORUM 1976*, Hornbeam Press, S.C.

A CLOZE STUDY OF VERB USAGE IN CONTEXT:

ANGLOS AND CHICANOS

Betty Lou Dubois

New Mexico State University

INTRODUCTION

Despite an occasional anachronistic effort such as Haugen's for Norwegian-American, the study of ethnic dialects in the United States did not become widespread, did not attract a broad range of students, until some time after World War II, when the civil rights movement had as one of its results the study of Black English. With the rise of a similar effort within the Hispanic community has come a serious look at the English spoken by such groups. In the Southwest, of course, overwhelming numbers of Hispanos are of Mexican extraction, coming increasingly to be called Chicanos, although certain adults across the age spectrum reject that appellation because of its political overtones. Nevertheless, it is now the custom to call the ethnic English of Mexican-Americans Chicano English.

In a recent paper, Professor Jacob Ornstein and I (1977) attempted to survey the field of Chicano English to show the kind of work that has been done and the kinds that still remain to be done, the latter category far larger than the former. We made the point that, for example, in the recently issued Teschner, Craddock, and Bills Spanish and English of the United States: A Critical, Annotated, Linguistic Bibliography (1975), there is not a single subhead for the English of any Hispanic group, although there are for Chicano, Puerto Rican, and Cuban Spanish. In the supplement to the bibliography (Bills, Teschner, and Craddock, 1977), there are again extremely few papers and collections which deal directly with the English of Mexican-Americans. Of those that touch upon the subject, especially the earlier ones, many are programmatic; others are restricted to lists of a few phonemes or morphological/syntactic items which contrast with standard English, so called "errors." Again and again, it is said that Spanish has no /ɣ/ but does have /x/ (but no one attempts to explain why many Chicanos say /xə/ instead of the expected /xə/). The reader could easily draw the false conclusion that Chicano English is nothing more than a collection of errors. At a higher level of language behavior, there has been, to my knowledge, little recognition that Chicano English is potentially differentiable through phenomena which can in no sense be categorized as errors in the way the absence of morphemic endings customarily is.

PURPOSE OF THE STUDY

Students of Chicano English are familiar with the Sociolinguistic Study of Southwest Bilingualism (SSSB) of the University of Texas-El Paso (UTEP), the language portion conducted under Jacob Ornstein, a pioneer in the field. Among the corpora available from the SSSB is the V Series, the part that concerns us consisting of ninety English language essays written by the thirty Spanish surnamed students of a random stratified sample of unmarried undergraduates regularly enrolled at UTEP. Each student in the sample wrote three essays, choosing one topic from each of three groups of topics which increase in degree of abstraction. My study (Dubois, 1975) of the essays of the V Series (and of those of the twenty-six students of the incomplete AC Series) looked first at the overall rhetorical effectiveness of the writing and found some very good as well as some very bad authors in the V Series. In addition to the expected in grammar, spelling, and punctuation, I inventoried selected stylistic features such as inversion of subject and verb. Intuitively, I found that there was more of such inversion than might be expected in the writing of an equivalent group of Anglos, but I had no way to test my insight, since precisely one of the problems in the study of Chicano English is the lack of adequate information on the language behavior of equivalent Anglos.

As I read and reread the V series essays, which were written in a university classroom and handed in largely as first composed, a style I will call formal unedited, I was struck by certain passages in which content verbs were repeated often enough to obtrude. As a practicing TESOL teacher, I am aware that the use of the proverb do is a basic element of competent English, one difficult to acquire for speakers of other languages. Hence, obtrusive verb repetition can be diagnostic of a real second language problem. Repetition for lack of synonyms can also be evidence of a genuine second language learning situation, although of course it can be indicative of a general educational deficit as well. More relevant here is the possibility that such repetition could be a dialectal stylistic feature.

The purpose of the present work, then, is to study a higher level stylistic feature, verb repetition in context. In order to test whether the patterns of repetition found to be used by the Spanish-surnamed are features of some potential Chicano English rather than of the English shared with their peers and correlated with social factors such as age, education, geography or the like, the study includes groups of equivalent Anglo subjects.

CLOZE INSTRUMENT

My work with the written English corpus of the SSSB reacquainted me with the well-known limitations of using corpora for

the study of any but the most commonly occurring language features. Moreover, collecting a large body of essays from a number of students is a very time consuming process, besides being inefficient for work on a single item. I therefore determined to study verb repetition more directly than possible through free composition by means of a CLOZE procedure. From the V Series essays, I took eight passages in which I judged that verbs were repeated more often than I would expect in formal edited prose. After the first use of the verb in question in each selection, I deleted the first and second repetitions in seven passages; one passage had a fourth use which I deleted to make the only item of such length.

THE PILOT TEST

The eight passages from essays written by UTEP Chicano students, with blanks of uniform length substituted for second and subsequent obtrusive repetitions of content verbs, were administered to approximately thirty subjects, both Anglo and Chicano, in and around New Mexico State University. The pilot clearly demonstrated that a free response instrument would not provide the desired information, since some of the subjects found ways to alter the meaning of the passages and thus avoid the problem, to make ungrammatical responses, and the like. It did, on the other hand, provide a range of synonyms, deletions, and pronoun substitutions from which I constructed the revised instrument. Fortunately, I discussed the test afterwards with various of the subjects sufficiently to discover that features in the passages different from standard formal edited English caused some responses to be made on the basis of what went best with the passages as they stood, rather than on the subjects' own best judgments of style. The experience should serve as a caution to investigators who obtain responses to items written in a particular ethnic dialect from nonmembers of the group.

PREPARATION OF THE REVISED CLOZE INSTRUMENT

The passages as revised were presented in multiple choice form. After the first instance of a verb, repetitions were replaced by blanks; directly below each blank were five choices, each of them making a grammatically correct sentence and a semantically acceptable followup to the initial use of the verb, of the following types, as appropriate and possible:

- (1) repetition of the verb
- (2) form of do
- (3) synonym
- (4) nonsynonym appropriate in context
- (5) deletion or pronoun if possible, second nonsynonym or appropriate nonsynonym

The order of the responses was randomized, to produce items similar to that one which follows:

. . . and am now (9) _____ a larger one.

- (a) working on
- (b) on
- (c) making
- (d) ding
- (e) crocheting [original verb]

The eight passages were themselves randomized to produce four different orders.

The verbs represented on the test range from very common (read) to relatively uncommon (crochet). In fact, speaking of general education deficit, on the pilot test, one subject read croquet for crochet. The kind of sequence in the passages, sequence being defined as presentation of stimulus verb + following blanks, varied from single through double to triple (a suite of three single sentences). There was syntactic variety of the following kinds in the sequences.

- A. Involving finite verbs
 - (a) finite--finite
 - (b) ed--ing
 - (c) infinitive--finite
 - (d) finite--infinitive
- B. Involving only nonfinite forms
 - (a) ing--infinitive
 - (b) infinitive--infinitive
 - (c) ing--ing
 - (d) infinitive--ing

ADMINISTRATION OF THE REVISED INSTRUMENT

The four versions of the revised instrument were given in approximately equal numbers to various classes of students from all five colleges of New Mexico State University. After elimination of unusable questionnaires, two different groups of students were represented in the study: (1) one hundred seventy Anglos and eighty Chicanos regularly enrolled; (2) a group of twenty-nine Anglo and sixty Chicano first semester freshmen officially identified as educationally disadvantaged by their ACT scores and enrolled in the special COPE program. Although class time was used, participation in the experiment was voluntary, as the initial information on the cover page made clear, and some students chose not to complete the instrument. Participants were asked to supply information about native and other languages and maiden names, to assist in classification. Instructions for completing the blanks were as follows:

The following eight passages are taken from the actual writing of university students. The passages now contain omissions, indicated by lines. Please read each passage carefully and from the possibilities below each line, circle the one that sounds best to YOU. You may choose to leave the line blank if you wish. There are no "correct" or

"incorrect" answers. After you have finished each page, reread the entire passage to make sure that what you have chosen makes the best-sounding English to YOU.

A sample item followed with response already indicated.

RESULTS

The faculty of New Mexico State University are fortunate in having routinely available a unique service: the Department of Experimental Statistics has as part of its mission the offering of expert assistance in statistical analysis to the members of the faculty. The assistance ranges from simple confirmation of a technique already chosen to complete design and execution of the statistical workup of a project, where widely known techniques are not suitable. With the present study, the latter proved the case, in that none of the statistical methods ordinarily thought of were best for analysis of the CLOZE procedure used here. I am grateful to Professor Morris Southward, who generously spent a great deal of time to find the most suitable way to analyze the results.

I will not detail the methods that were rejected as inappropriate, but will say merely that, given the unequal numbers of subjects in the four subgroups (regular Anglos, regular Chicanos, COPE Anglos, and COPE Chicanos) and the complexity of the response scheme (each single sequence gave twenty-five possible responses), Professor Southward elected first to set aside the one example of three-response sequence and to deal with the remaining sequences as single ones. He selected the method of cumulative frequency of percentages as the best way to compare the unequal groups.

I will deal first with the regular students, reporting that Professor Southward was unable to find any statistically significant differences in the responses of the two groups, whether the sequences were analyzed individually or grouped in various ways. Thus, despite relative availability of synonyms, difference in syntactic function of the verbs tested, or occurrence in a passage with one, two, or three sequences, the Anglo and Chicano students tested at New Mexico State University who represented regularly enrolled students in all five of the colleges exhibited roughly the same behavior in the selection of verbs in passages in formal edited prose.

This is not to say, however, that there were no differences at all between the two groups. Table 1 displays the percentages of the three single sequences testing play, read, and speak. I have selected four percentiles for presentation: 25%, 50%, 75%, and 100%. In addition, I felt it important to indicate any sharp increase (10% or more) by an asterisk, and where the jump is not coincident with one of the key percentages, to indicate the cumulative percentage within the parentheses. As Table 1 indicates,

		PLAY		READ		SPEAK	
Responses		Ch	A	Ch	A	Ch	A
A repetition	AA	(14*)	(11*)	74	79	48	40
	AB			76			
	AC					53	
	AD			78			
	AE	49*	39*				50
B do	BA	54					
	BB						
	BC						
	BD						
	BE		50				
C synonym	CA						
	CB						
	CC						
	CD						
	CE						
D nonsynonym	DA					87*	74*
	DB						
	DC						76
	DD						
	DE	76					
E other	EA		79				95*
	EB						
	EC			100	100		
	ED						
	EE	100*	100*			100	100

Table 1. Single Sequence Responses of Regularly Enrolled Students

there are scarcely any available synonyms for read and large, nearly equal response numbers are reached by two repetitions. There is roughly even drift to 100% response at EC. For speak, not grossly different percentages respond with two repetitions, with a little difference in second response and slightly more variation at the end. With play, there is a 10% difference in group A, but the largest percentage in both groups is reached by a sharp jump into response AE. The distribution tends to even out.

With the double sequences, I can still say that the first response of Chicanos to crochet and speak is more than fifty percent repetition. Anglos repeat crochet and sew less than Chicanos, but repeat speak more. Across all sequences discussed so far, the following trends emerge: Chicanos tend to repeat slightly more and, astonishingly to me, to use do more than their Anglo peers. On the other hand, Anglos tend slightly more to make response E: pronoun, omission, etc., that is, to alter the syntax more than the Chicanos tested. As to the second in the double and the second and third in the triple sequences, responses of the two groups tend toward the same slots, with, of course, nonsignificant differences in percentages. In the one three response sequence, I was startled at the similarity of the response clumping in 125 possibilities. The four checkpoints are tabulated in Table 2.

Response	Chicano	Anglo
ABA	32*	28*
AEA	55*	52*
EAA	85*	82*
EEA	100	
EEE		100

Table 2. Regular Anglo and Chicano Responses in Three-Choice Sequence

The two COPE groups, that is, the educationally disadvantaged, behave like the regular groups on the single sequence testing repetition of the verb read, which points again to the lack of common synonyms. As Table 3 indicates, they behave similarly with respect to the first repetition of play, except that for the second blank the Chicano group tends to choose a synonym, while the Anglo group chooses the pronoun. Both groups bunch up in the first repetition of the verb speak but more Chicanos (75% being reached) than Anglos (50% being reached). With respect to the other varieties of response, the Anglo group was in both cases spread out further, i.e., they responded in more different ways than the Chicanos did. Neither group used do to any extent.

		PLAY		READ		SPEAK	
Responses		Ch	A	Ch	A	Ch	A
A repetition	AA			64	77		
	AB						
	AC	(24*)	31*	77			39*
	AD	38*				(74*)	50*
	AE		(44*)			78	
B do	BA	57*	56*				
	BB						
	BC						
	BD						
	BE						
C synonym	CA	(71*)					
	CB						
	CC						
	CD	81					
	CE		75				
D nonsynonym	DA	(95*)					78*
	DB						
	DC						
	DD					100	(89*)
	DE						
E other	EA			100*			
	EB	100					
	EC				100		
	ED						
	EE		100				100

Table 3. Single Sequence Responses of Educationally Disadvantaged Freshmen

When speak was tested in a double sequence, different behavior resulted. 67% of both groups was recorded for the sequence speak/do. Chicanos then select synonym for the second blank after repetition and 11% of the Anglos choose deletion after the blank. The other two double sequences find the Chicano group again repeating more than the Anglo and subsequent responses showing less variety. In the second sequences, Anglos tend to repeat more than Chicanos, but the remainder of the response distribution of the two groups is more similar than in the first portion of the sequence, i.e., the differences tend to iron out in longer sequences. In the triple sequence, both groups have about 25% double repeats. A large percentage jump to 63% is reached for Anglos at repeat/pronoun. The second sequence finds the Chicano group repeating more, with the rest of the distribution fairly evenly balanced. There is very little repetition in the third sequence, and the distribution is quite similar for the two groups.

Now, for a comparison of the two COPE groups with the regular Anglos. Responses at the top of the column, i.e., at repetition, are not too dissimilar, except that COPE Chicanos avail themselves less of the pronoun option than either of the other two. For speak, the Anglo groups have about the same number of repetitions, except that the regular group has more second repetitions. The Chicano group repeats noticeably more than the others and in this instance uses deletion more. The generalization for the balance of these responses in single sequence is that the regular Anglo group's are more evenly distributed across the possibilities, the COPE Anglos' less so, the COPE Chicanos' still less. In double sequences, there is both more repetition of the verb and less use of do in the COPE groups than in the regular Anglos, again with a little less spread across the responses in the COPE students. In the second of each pair of sequences, there are sharper and generally more numerous jumps with the COPE groups, whose performances resemble each other more than they do the Anglo group's. In the triple sequence, both Anglo groups are somewhat similar at the beginning, the Chicanos different, partly in a smaller number of repetitions. In the second sequence, the Chicano repetition pattern approximates the regular Anglo more than the COPE Anglo does, but by the time the 80% range is reached, the groups are behaving similarly. By the third of the three sequences, all groups prefer a synonym, the regular Anglos making sharp jumps there, and only then repeating. Behavior on this third sequence of the triple is more similar than in the first and second parts, except that once again the Anglo groups resemble each other more than the COPE group.

SUMMARY

Two groups of regularly enrolled students from all five colleges of New Mexico State University were given a multiple choice CLOZE instrument to investigate patterns of verb repetition in context. The groups of regular Anglo and Chicano students demonstrated roughly the same patterns, except that the Chicanos tended to repeat

verbs and use do more and Anglos to use pronoun, deletion, etc. more. The COPE groups of educationally disadvantaged freshmen differed from the regular Anglo group in that they made more repetitions, the Chicanos generally more than the COPE Anglos. As for total variety of responses, i.e., spread of responses across the twenty-five possibilities, regular Anglos displayed greatest spread and smoothest spread, followed first by COPE Anglos and then by COPE Chicanos. The differences tended to decrease with multiple sequences.

CONCLUSIONS

Labov (1973) has stated the necessity of relating language experiments to the real world of language behavior. I have attempted to follow Labov's dictum in two ways, initially, by using passages taken from actual student writing although the pilot test indicated the necessity for slight editorial revisions. Moreover, during their completion of the revised instrument, students were observed not only choosing alternatives, but also rereading, i.e., editing, the passages. The choosing of verbs and subsequent editing in formal writing approximates a real world student language use, writing and editing term papers, reports, and essay exams.

The strong similarities of verb repetition patterns between the regular Chicanos and Anglos indicate that when the factor of education is the same, so is language performance, at least in selecting for suitability in passages of formal edited English. There is no implication that the groups would produce the same variety of English, although it is my opinion that it would be highly likely, at least at New Mexico State University. At other universities with demographically different Chicano populations, the case might easily be different.

The COPE groups of educationally disadvantaged freshmen are different not only from the Anglo group, but from each other. In the case of the COPE Anglos, one can attribute the difference to educational deficit. With the Chicanos, there is undoubtedly a factor of educational deficit; it is also possible that their patterns of verb repetition are a higher level feature of an ethnic dialect of English. What is needed now is follow-up studies to demonstrate that fact.

REFERENCES

- Bills, Garland D., Jerry R. Craddock, and Richard Teschner. 1977. "Current research on the language(s) of U. S. Hispanos." Hispania. 60 (May): 347-358.
- Dubois, Betty Lou. 1975. "A plan to study the written English competence of Chicanos." In Gina Cantoni Harvey and M. F. Heiser, eds. *Southwest languages and linguistics in educational perspective*. San Diego: Institute for Cultural Pluralism of San Diego State University.
- Labov, William. 1973. *Sociolinguistic patterns*. Philadelphia. University of Pennsylvania.
- Ornstein, Jacob, and Betty Lou Dubois. 1977. "Mexican-American English: Prolegomena to a neglected regional variety." In Robert J. Di Pietro and Edward L. Blansitt, Jr., eds. *The third LACUS forum 1976*. Columbia, S.C. Hornbeam.
- Teschner, Richard V., and Jerry R. Craddock. 1975. *Spanish and English of the United States: A critical, annotated, linguistic bibliography*. Arlington, Va.: Center for Applied Linguistics.

THE NUMERICAL NAMING OF PEOPLE

Allen Walker Read
Columbia University

Most members of this audience have noticed, I suspect, the increasing use of numbers to identify people. Inasmuch as the computers are now inevitably with us, we are accepting this trend with varying degrees of equanimity. But we love our names, and the displacement of them by numbers tends to arouse disquiet and displeasure. The incoming vice-president of this Association, Ernst Pulgram, has voiced the predominant attitude that we have to this situation, as follows:

It is quite feasible, though not always practical, that people be identified and called by numbers, as indeed they are when they join a body in which their individuality becomes, perforce or perchance, diminished in importance and of less interest, as, for example, in prison, in military units, or on the time clock of a workshop.... Loss of name and its replacement by a number are felt, by most people, as a deplorable, often dishonorable, but sometimes necessary, loss of individuality. If a man is not important enough to own a name, if nobody is interested in knowing his name, he feels that he has entered, in his living days, upon the path to oblivion.¹

On the other hand, a defense can be found for the contrary view, and this was expressed by Robert Ian Scott of the University of Saskatchewan. He has written:

Index numbers on credit cards, licenses, and insurance policies identify us less ambiguously than our names do. We need such numbers because Ralph C. Nelson 14442 labels one individual, and Ralph C. Nelson 26375, quite another one. Some say they resent "being numbers", but generally such numbers increase the possibilities of our lives by unconfusing them, and by reminding others that we are not precisely like anyone else, thus making it easier to treat each case individually.²

In this paper, I wish to give something of the background of numerical naming, and to document the tendencies that are now at work.

In historical times the Romans used only eighteen given names (praenomina) and have been much criticized for this paucity. Of these eighteen, three are numerical---Quintus, Sextus, and

Decimus. It is not clear why these particular numbers were the survivors.³

Anthropologists have found that some early cultures have been secretive about their names. In Chinese culture, some families have limited themselves to calling their children "First", "Second", "Third", etc.---an expedient that would circumvent the evil spirits in getting hold of their "real names".⁴

Similarly the Apaches in southern Arizona, when the American soldiers arrived in the 1870s, were stubborn about revealing their true names. The Army had to give the Indians serial numbers and many of them were then adopted as names. One of them, R-14, became a wealthy rancher, and a peak in Arizona was named R-14 for him. After the Army moved out, the Indian Service attempted to substitute other names, but many of the Indians insisted on keeping their numbers.⁵

The trend toward the use of numbers for people became apparent in the course of the nineteenth century---as for example in police usage in Paris. As early as 1839 an Englishman thus criticized the practice there:

This ticketing system ... much resembles the system adopted at Paris, where a poor old apple-woman is numbered 13,194, and her apple stall 17,643---her dog, who is blind, and asks for alms, with a leather saucer in his mouth, 33,275; so that the police agent, if he has to make a charge against the aforesaid dog, begins his complaint as follows---"Monsieur le Commissaire, as I was proceeding down the Rue St. Honore, in the Section 36 of the district D, I saw 33,275 seated near 17,643, which was presided over by 13,194." And then follows the charge of the dog-begging, and of the policeman reproving, and of the old woman getting angry, and of the dog barking, and of the table falling, and of all being taken into custody; the result of which is, that 33,275 is ordered to beg no more, 17,643 to fall no more, and 13,194 to scold no more a policeman such as 263, belonging to section Y of the arrondissement, No. IX. Well now, for my part, I hate this numbering and ticketing system.⁶

American folklore has presented a cycle of reports on numerical naming---often attributed to a family named Stickney. The earliest account that I have found is from a New York journal in 1839, as follows:

We know a man "out west", by the name of Stickney, who had so many sons that he couldn't find names for them all, so he distinguished them in this manner---One Stickney, Two Stickney, Three Stickney, and so on, from the oldest to the youngest. For instance, in giving out his orders, he would say: "Here, you One Stickney, go and put out this gen'p'lans horse; Four Stickney, go to

school; Ten Stickney, draw a bucket of water, and tell Fifteen Stickney to help you.⁷

Thirteen years later, in 1852, the Stickneys again turned up in the account of a Scottish traveler, John Regan, who wrote:

... a gentleman on the banks of the Miami, of the name of Stickney, designated his children in numerical order, commencing with the first born---One Stickney, Two Stickney, and so on down to the infant, who is called Five Stickney.⁸

Another Scottish traveler, David Macrae, in 1870, transferred the event to Michigan, and included further embellishment. As he wrote:

In one family in Michigan the sons were christened One, Two, Three, and the daughters, First, Second, Third ---a safer method than was adopted by the Vermont couple who called their second child Finis, thinking it would be the last, and who, when a girl and two boys appeared afterwards, had to call them Addenda, Supplement, and Last Appendix.⁹

The oral transmission of this material was summed up in 1889 by John S. Farmer, the lexicographer, in this account:

[A Mr.] Stickney, in one of the New Londons to be found in fourteen of the American States, thinking to perpetuate his honorable patronym, hit upon the simple, though monotonous expedient of dubbing his firstborn Stickney No. 1. This child was not the only son of his mother; for in due course "another Richmond held the field" in Stickney No. 2. The process continued until Stickney No. 12 scored honors all round.... Another citizen of the Stars and Stripes, thinking his duty to his country fully accomplished, labelled his fourteenth girl "Finis". Alas! for human hopes; ... his quiver grew "more full and overflowing" with yet another hostage to fortune, whom, in sheer desperation and shamefacedness, he could only register as "Addendum".¹⁰

Similar namings have been reported in recent years. A London paper of 1956 printed this report from America: "A Tennessee, U.S.A., father of 16 [children] admitted he'd run out of names when his wife presented him with their sixth set of twins. He said he might call the new pair 'Seventeen' and 'Eighteen'."¹¹ In the New York Times in 1976 a contributor reminisced about a naming incident: "... one of my earliest companions became a boy named 'Eleven'. Eleven was his parents' 11th-born child. When he came along they were at a loss for a name they hadn't already used, and so they decided to improvise. I don't know what became of Eleven."¹²

Joking about numerical naming has been rife. Noel Coward is the source, perhaps, of a well-known witticism. He wrote to T. E. Lawrence, who was then in the Army, in a letter of August 25, 1930: "Dear 338171 (May I call you 338?)."¹³ Another such joke is told of two inmates at Sing Sing who had a fight, and the one accused of starting it said: "He called me a dirty number."¹⁴

But joking is far from appropriate with regard to the numbers as used in authoritarian regimes. In central Europe, the number of the "work permit" or *karte* came to represent one's status as a person. The aria, "My Name Is a Number", in Menotti's opera *The Consul*, has an overwhelming bitterness.

The survivors from the Nazi death camps have a problem with their numbers. One of them wrote as follows to a psychological counselor, printed in a New York City newspaper:

Numbers were a very convenient thing and also a symbol of Nazi barbarism. You give a group of people numbers and you haven't got anything to do any more with human beings, but with statistics. It has already been 13 years since my liberation, and every summer when the tattoo on my arm is noticeable, I don't know what to do about it. I am neither proud nor ashamed of it.

Many American friends ask me (not to embarrass me in any way) why I don't take it off. Many times I ask myself that same question. What do I need it for? I have two brothers and friends with numbers on their arms. None of them has taken them off.¹⁵

The counselor pointed out the ambivalence of her attitude, even though she was "neither proud nor ashamed". However much the tattooed number had been a mark of shame when it was inflicted, it had turned into a badge of honor as a memorial.

Let us turn now to a consideration of the movement to establish a numbering system for all people. This has been especially strong in the various bureaus of vital statistics. The earliest general plan was undertaken in Canada in 1918, when the Dominion Bureau of Statistics inaugurated a coordinated system among the provinces for the unification of birth records. Then in England, early in the Second World War, a "national register" was formed, and Sir Sylvanus Vivian, Registrar-General, pushed forward to a "complete peace-time national register". All children were to be assigned a code number at the time of registration of their births.¹⁶

The decade of the 1940s was especially active for the United States. In October, 1944, the "Registration Executives of North America" held a meeting in New York at which the Bureau of the Census laid before them a "Plan for the Coordination of Vital Records and Vital Statistics in the United States". It got under way, and a "Vital Statistics Council for the United States" was formed.¹⁷ A leader in this development was Mr. Lucien Benepe, the director of Montana's bureau of vital statistics, and it was wide-

ly reported that Montana was the first state to adopt a law requiring that every baby at birth should receive a number that would not change during his lifetime and would be discarded only at death. In an effort to get a copy of this law I wrote to the State Law Librarian, and she assured me that Montana did not have such a law.¹⁸

Nevertheless, individual state bureaus went ahead with their plan. A report issued in July, 1949, by Paul Weis, Assistant State Registrar of Wisconsin, gave the following details:

Upon agreement between the vital registration authorities of the 48 states, the District of Columbia and independent city registration areas and state health authorities every birth certificate for births occurring on or after January 1, 1949, is receiving a state birth number. In Wisconsin no state numbering system has previously been in effect, although it has been felt desirable for practical purposes and its application offers no difficulties.

The state numbering system calls for a number in three segments: the first three digits for Wisconsin, namely, 148---indicating 1 for the United States, 48 for the state of Wisconsin; followed by a second segment---49---for births in the current year; and by the third segment indicating the individual's consecutive number.

The system has been adopted in the anticipation that eventually the public will find it convenient and desirable to have a system for linking together all of the important personal records regarding any individual by means of his unique birth number. Innumerable municipal, state and federal government agencies now use numbering systems of a wide variety and envision decided convenience and simplification in the possible future use of the birth number as a means of personal identification and record linkage.¹⁹

This plan made the Social Security authorities uneasy because of a duplication of numbers, but Mr. Weis felt that a linking of records could be accomplished. He concluded his report on a sanguine note:

There has not as yet been any definite plan as to how and when (if ever) the birth number will be put to any of its many potential uses. All that we now know is that the generations born this year of 1949 and hereafter will have a birth number, which will make any number of its potential uses possible.

The social security numbering took a leap forward in the year 1961, when Congress passed a bill requiring that every taxpayer must use a number in his tax returns. The Internal Revenue Service

needed this in its switch to an automatic electronic data-processing system.²⁰ The nation was blanketed with a circular from the United States Treasury Department with the heading, "Do You Have a Social Security Number?" and those without numbers were required to get them.²¹ This met with little opposition, as most citizens stand in awe of the IRS.

Strong opposition to numbers did develop in the next year, 1962, on the basis of another issue. Among the telephone companies a change was being made to drop the exchange names and to substitute numbers. In San Francisco, the Anti-Digit Dialing League, a non-profit organization, was formed and received national publicity. It distributed a pamphlet entitled Phones Are for People, containing the message, "If you join with [us] in the struggle to retain meaning and identity in an increasingly impersonal society, we urge you to contribute to the cause."²² The phrase "creeping numeralism" was coined during this dispute.

A leader of the movement was Dr. S. I. Hayakawa, of San Francisco State College, now a Senator from California. In defending the movement he remarked: "People ask me, 'Are you not trying to stand in the way of the inevitable?' My answer is: In social matters there are very few things that are inevitable, unless we agree they are."²³ Chapters of the Anti-Digit Dialing League were formed in Palo Alto, Berkeley, Sacramento, Los Angeles, and Bloomington, Indiana; but by now it appears that the cause is lost.

Skirmishes have been continuing in recent years, and resistance comes from many quarters. A husband and wife of West Islip, Long Island, refused to register their four children for Social Security numbers out of fear of the "wrath of God". They testified that they were "Messianic Jews", who believe that Social Security numbers are "the tools of an Anti-Christ". They said that if their children were given numbers, "their spiritual well-being and chance to enter Heaven would be seriously jeopardized". Because of their refusal, the family was removed from the welfare rolls in July, 1976. In deciding the case, Judge Jack B. Weinstein issued a 35-paged opinion accepting their sincerity and stating: "A religious belief can appear to every other member of the human race preposterous, yet merit the protections of the Bill of Rights." He ordered them re-instated on the welfare rolls and observed: "The deleterious effects of their action on the welfare system is miniscule."²⁴

Another curious case arose in 1976 when a teacher in Cass County, North Dakota, named Michael Herbert Dengler, petitioned the court to change his name to "1069", on the ground that those numbers express his relationship with nature, time, and the universe. He said that 1 represented his picture of nature which manifests itself as one individual, the zero showed his relationship with time and movement through life, the 6 "was equal to the relationship he has with the universe", and 9 "indicates the relationship he has to essence".²⁵ The judge denied his request, but the case was carried to the North Dakota Supreme Court. There Associate Justice Paul M. Sand admitted that "innovative ideas, even though bordering on the bizarre, are frequently encouraged

and may be protected by the law"; but he refused to overturn the district court's rejection, with the opinion: "We are satisfied that the Legislature, in giving authority to the courts to change a name, had in mind a name as understood and defined by common law and did not include change from a name to a number."²⁶

Practitioners in the field of mental health have also delivered their opinions. Thus Dr. Leo Goldberger of the New York University Research Center for Mental Health has stated: "Long series of numbers, such as Army serial numbers, have come to connote loss of individual identity: one becomes---to add insult to injury---not only an insignificant cog in a great machine, but anonymous as well."²⁷

Certain members of the Women's Liberation movement have objected to the naming of hurricanes after women in alphabetical order, but they have met opposition. One such person wrote to a newspaper:

Tell those women libbers to knock it off. Each season I keep hoping the National Weather Bureau will name a hurricane after me, and now, there is a move afoot in Washington to force the Bureau to change the system and name hurricanes after men, mythological characters or, worse yet, numbers.

... [Signed:] Hurricane Charline²⁸

The pressure is still great to have a "national ID card" for the ease of electronic data processing. A "Federal Advisory Committee on False Identification" was appointed, consisting of 75 government officials, business executives and other private citizens, and they studied the proposal for eighteen months. On December 7, 1976, they issued an 800-paged report opposing such a system, for such centralization, they felt, would make the problem worse. As the report said: "Criminals could reap benefits far greater than they obtain under the current multifaceted system of identification."²⁹

Two months later another proposal was published in the New York Times by Ralph Love, for the establishment of "a permanent, up-to-date record of each American citizen and legal resident alien."³⁰ This was satirized by a writer ten days later who said that Mr. Love had omitted a necessary paragraph from his plan, namely: "The number assigned to each citizen shall be tattooed on the left arm at birth."³¹

In the course of the search for the murderer known as "Son of Sam", the officer in charge, Inspector Timothy Dowd, declared: "I tell you frankly, if the police got everything they wanted to make their job easy, everyone would have a number tattooed on his arm and there'd be no freedom at all."³² He was well aware of the rivalry between claims to the right of privacy and claims of law enforcement, which could lead to a police state.

Only a few weeks ago, an influential businessmen's group of New York City, the Economic Development Council, issued a report on the urgent problem of illegal aliens in this country, said to

number more than eight million. Their chief suggestion dealt with the numerical naming approach. As the report said:

We suggest here universal social security numbering at birth, coupled with a program to enroll all citizens, with numbers cancelled at death. Once established, obtaining a social security number after birth would require documentation. Illegal aliens would not be free to use another's number. Duplicate numbering would also be prevented. Ultimately all American citizens will require a social security number; there is already widening use of numbers for record keeping. Requiring it at birth would not entail any great personal burdens.³³

Thus the pleadings for numerical naming continue to the present time.

It remains to ask the question, does the theory of proper names offer any guidance for us? Can we expect help from the science of onomastics? We are fortunate that two LACUS members have written important monographs in the field---Ernst Pulgram's Theory of Names in 1954 and John Algeo's On Defining the Proper Name in 1973.³⁴ Both of them wrestle manfully with the problem of the nature of the proper name, but I do not find any material directly on whether a number used as a designation can be called a proper name.

Perhaps we should go back to a footnote in Sir Alan Gardiner's little book, The Theory of Proper Names, of 1954. In discussing such names as Oxford, Mont Blanc, and Buenos Aires, he states: "The fact that these names have some significance does detract a little, but only a little, from their purity. The purest of proper names are wholly arbitrary and totally without significance."³⁵ Would we not conclude from such a comment that a number would be the purest possible proper name? Numbers are free from etymology, unless we are dealing with a numerologist's fancy, such as Dengler's "1069".

But do we want so-called "purity" in proper names? Here we come to a decisive point. We learn from information theory that it is desirable to have what is technically called "noise" for ease of assimilation. "Pure" information is difficult to handle. It may be that numerical names need "noise", or "redundancy", in this technical sense. My social security number, 108-26-9774, needs the noisiness that the name Allen Walker Read, inseparable from its social background, affords. People in general require these human factors, and therefore are willing to hold out, at least for a while, against "creeping numeralism".

NOTES

¹Ernst Pulgram, "Theory of Names", in Beiträge zur Namenforschung, V, No. 2 (1954), p. 32.

²Robert Ian Scott, "Is-less and Other Grammars", in General Semantics Bulletin, Nos. 38-39-40 (issued 1976), p. 42.

³Harper's Dictionary of Classical Literature and Antiquities, ed. Harry Thurston Peck (New York, 1897), art. Nomen.

⁴Raymond D. Jameson, in Funk & Wagnalls Standard Dictionary of Folklore, Mythology and Legend, ed. Maria Leech (New York, 1950), II, 782.

⁵Byrd Howell Granger, "Naming: in Customs, Beliefs, and Folk Tales", in Western Folklore, XX (Jan., 1961), 32-33, based on information from informants in Arizona.

⁶"By a Fine Old English Gentleman", in The Corsair, I (May 11, 1839), 142, col. 1. Later an American traveler in England made a similar complaint; see James B. Converse, A Summer Vacation; Sketches and Thoughts Abroad (Louisville, Ky., 1878), p. 179: "At only two of the British hotels was I asked for my name. In all others, travellers are designated (like penitentiary convicts) by numbers."

⁷New Yorker (quarto), VII (August 10, 1839), p. 32, col. 3.

⁸John Regan, The Emigrant's Guide to the Western States of America (2nd ed.; Edinburgh, 1852), p. 108, footnote.

⁹David Macrae, The Americans at Home (Edinburgh, 1870), II, 330, footnote.

¹⁰John S. Farmer, Americanisms Old & New (London, 1889), p. 383.

¹¹Reveille, April 13, 1956, p. 24, col. 2.

¹²Russell Baker, in New York Times Magazine, August 29, 1976, p. 4, cols. 3-5.

¹³Cited in the New Yorker, April 26, 1947, p. 45, col. 1. Cf. the report in the New York Post, December 31, 1964, p. 10, col. 2: "The Humor Societies of America have a new comedian from Outer Space named Snyder 2789654765555 (nickname 2789)."

¹⁴Earl Wilson, in the New York Post, September 23, 1952, p. 17, col. 2.

¹⁵New York Post, August 12, 1958, p. 6, cols. 1-2, in column of Dr. Rose N. Franzblau.

¹⁶John T. Marshall, "A New Pattern in Registration Concepts", in Canadian Journal of Public Health, March, 1945, as reprinted in Vital Statistics--Special Reports, XXIII, No. 4 (July 3, 1945), 79-86.

¹⁷Ibid., p. 83.

¹⁸The premature report is given by Elsdon C. Smith, The Story of Our Names (New York, 1950), p. 63.

¹⁹Paul Weis, "Notes on Vital Registration", in Wisconsin State Board of Health Quarterly Bulletin, VIII (July, 1949), 471-72.

²⁰E.g., Robert Metz, "Taxes by the Numbers; Link with Social Security Expected to Round Up Many Delinquents in U. S.", in New York Times, Oct. 2, 1961, p. 47, cols. 5-6. Also "Every Taxpayer

to Get a Number", *ibid.* October 8, 1962, p. 35, col. 2.

²¹U. S. Treasury Department, Internal Revenue Service, Document No. 5414, accompanying Form 3227. Investment companies sent out flyers with the heading, "Important Message to our Shareholders".

²²Phones Are for People (San Francisco: Anti-Digit Dialing League, 1962), p. 10.

²³New York Times, June 24, 1962, p. 51, col. 1. Cf. also Fred J. Cook, "The New Numbers Game", in New York Times Magazine, September 23, 1962, p. 31 ff. As a contribution to the humor market, see How to Survive the Numbers Explosion, by 1523514 1615165 23513114 (San Francisco: After 5 Publishing Co., 1962), pp. A-Y, with essays on topics such as "The Art of Number-Dropping", and "How to Choose a Clever Number for the New Baby".

²⁴New York Post, March 4, 1977, p. 12, cols. 3-4.

²⁵Kansas City Times, Jan. 12, 1976, as quoted in the American Name Society Bulletin, No. 49 (April, 1977), p. 15.

²⁶New York Post, Nov. 9, 1976, p. 63, col. 1.

²⁷Quoted in New York Times Magazine, Sept. 23, 1962, p. 31. The issue is well stated by Robert M. Landau, "Name or Number--- Which Shall It Be?" in Names, XV (March, 1967), 12-20.

²⁸New York Post, August 8, 1977, p. 38, cols. 1-2, in the "Dear Abby" column.

²⁹New York Times, Dec. 8, 1976, Sect. A, p. 20, col. 6.

³⁰New York Times, Feb. 13, 1977, Sect. IV, p. 16, col. 5.

³¹Ruby O. Richman, *ibid.*, Feb. 23, 1977, Sect. A, p. 22, col. 5.

³²New York Times, May 21, 1977, p. 35, cols. 2-4.

³³Economic Development Council of New York City, Inc., George Champion, Chairman, The Illegal Alien and the Economy, 20 pp. mimeographed (1977), p. 2. Also New York Post, April 26, 1977, p. 18, cols. 4-5.

³⁴Ernst Pulgram, as in footnote 1 above; John Algeo, On Defining the Proper Name, University of Florida Humanities Monograph No. 41 (Gainesville, Fla., 1973), 94 pp.

³⁵Sir Alan H. Gardiner, The Theory of Proper Names (2nd ed.; Oxford University Press, 1954), p. 19, footnote 1.

VIII

**TEXT LINGUISTICS:
DISCOURSE ANALYSIS BEYOND THE SENTENCE**

A CALCULUS OF COHESION

Brian Phillips
University of Illinois at Chicago Circle

In theories of the structure of coherent discourse two approaches can be discerned. One reflects the influence of transformational generative theories of syntax, the other is analogous to proof discourse in mathematical logic. The two are not necessarily mutually exclusive. In this paper I want to examine the validity of these approaches to see the phenomena that they seek to describe and how well they achieve this purpose. It will be shown that the former is formally false but that the phenomenon that it seeks to characterize is not accounted for by logical models.

My present concern is not with the syntactic consequences of a unified sequence of sentences, such as the use of definite and indefinite articles, premoninalization, etc., for which Grimes (1975) uses the term "cohesion". Rather I will be examining cohesive content, what it is acceptable to say next, not the precise form that it should take, e.g.,

- (1) John saw Mary trip. { She was always clumsy.
*She liked chocolate chip cookies.
She was crossing the street.
Bill saw it too.
She grazed her knee.

Transformational Models

Lakoff (1972)* seeks to show that "any adequate theory of the structure of fairy tales must share many formal properties with the transformational linguistic theory that has so far been proposed by Noam Chomsky and his coworkers" (P. 128). Using Propp's (1968) analysis of Russian fairy tales, Lakoff develops a grammar that has a base component of context-free rules that produce the "deep structure" of the tale, as, for example, in Figure 1. The terminal symbols of the context-free grammar (CFG) are different in nature from those of a sentence grammar where they are atomic syntactic categories. The discourse symbols are semantic items that "would be taken from an adequate theory of semantic structure" (p. 138) with blanks for filling in the particular subjects and objects. In other words, they are not strictly atomic, although they can be so treated by the base component. Transformations describe co-occurrence phenomena, e.g., "if the villainy in a tale

* It should be noted that Lakoff wrote the paper in 1964 and is no longer in total agreement with the views he expressed then.

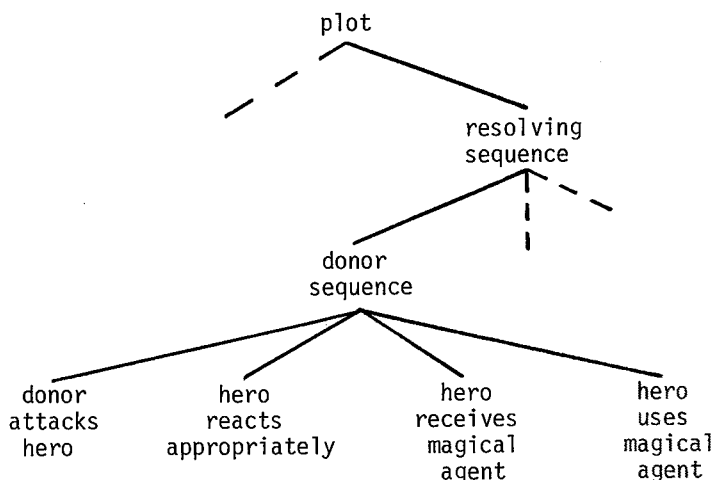


Figure 1

is a kidnapping, the person kidnapped will either be the hero's sister, a priest's or a merchant's daughter, a princess, the hero's bride, etc., but not the king, a dragon, a wicked prince, an old hag, etc." (p. 139), the conjoining of tales, and the embedding of tales within another tale. Lakoff is claiming only a theory of fairy tales. Whether it is adequate for this is one point to examine. A second point is to consider whether the model is appropriate for general discourse.

The base component of fairy tales is a CFG which is a "4-tuple (V_N, V_T, P, S). The symbols V_N , V_T , P , and S are, respectively, the variables, terminals, productions, and start symbol. V_N , V_T , and P are finite sets. We assume V_N and V_T contain no elements in common" (Hopcroft & Ullman, 1969, p. 10). Propp observed that the number of "functions" (the leaf node labels in Figure 1) is limited. Consequently the finite set conditions of a CFG are satisfied, according to Lakoff. If the same formalism is to be extended to general discourse, the same condition must be true. It can readily be shown that it is not; in fact the assumption for fairy tales may also be suspect. Although the number of terms possible at the highest nodes in a derivation tree may be finite, nearer to the leaves almost any number of items could appear. For example

- (2) Less well established are the few stories of Oliver's babyhood . . . a tame monkey seized Oliver out of his cradle . . . the four-year-old Oliver bloodied the nose of the two-year-old Prince Charles Stuart (Fraser, 1973, p. 15).

The structure of this is as shown in Figure 2. There is an open set of lexical items similar to "story": "idea", "tale", "thought", "accident", etc., that could appear in deep structures. Further the distinction between terminals and variables is not clear cut, as

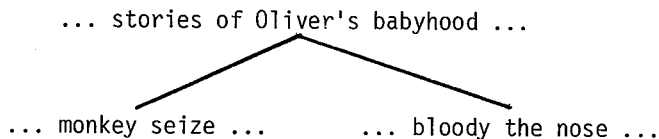


Figure 2

Rumelhart (1976) shows. Given brief stories, subjects produce summaries of varying degrees of detail that correspond to a choice of different levels in the hierarchic deep structure. A fragment of a structure is given in Figure 3 (not in Rumelhart's notation). Corresponding summaries are:

- (3) Level 1
Three daughters were saved by three heroes.
- (4) Level 2
(The selection of method is not expressed.)
(The locomotion is not expressed.)
Three heroes rescued the daughters.
The Czar rewarded the heroes.

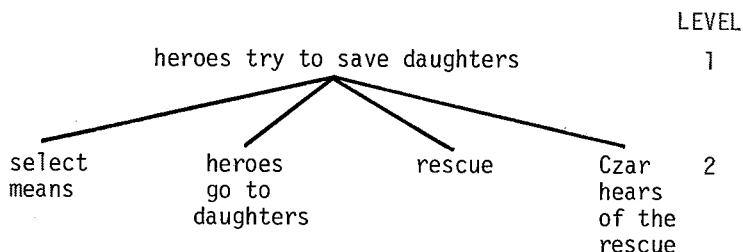


Figure 3

This is counterevidence to the claim for a set of uniquely terminal symbols in the CFG. Thus it is not valid to use a CFG as a model for the deep structure of general discourse.

Van Dijk (1972) uses context-free rewrite rules to generate "macrostructures", the deep structure of discourse. The rule set also contains some with "blank" slots on the right-hand-sides for the insertion of participants, as for Lakoff. Figure 4 shows the macrostructure for a three paragraph discourse from *Mythologies* by R. Barthes. The three main propositions are: (1) The critics say that if they don't understand a difficult philosophical work, this work is bad; (2) This argument is based on a general terrorist myth for masking one's own ignorance; (3) But the philosophers understand the ignorance of the critics. Van Dijk's view of the deep structure of discourse is different from Lakoff's. Lakoff uses abstract terms that cover segments of the tale whereas van Dijk's is more a selection of prominent propositions of the discourse. As neither has a system that actually realizes their underlying structures, it is not clear how important this difference is.

naturally formulated as a grammar, every sentence occurring in it is semantically as well as syntactically related to the rest in a specified way (p. 2).

Working in the domain of mathematical proof and mathematical English, Corcoran (1972) distinguishes two kinds of proof, linear and suppositional. "A [linear] proof of c from P is a finite sequence of lines ending with c , each subsequent line of which either is an assumption in P or is a logical axiom or is obtained from previous lines by a rule" (p. 7). In the following example we can see "modus ponens" being used:

- (5) Mr. Ford said during a political rally that he would be campaigning in "every" primary next year . . . If Mr. Ford means what he says--and he seemed to be disclosing his plans, not engaging in hyperbolic pep-talk--he will be away from Washington a great deal, an unusual way for an incumbent President to campaign. (New York Times)

"Modus tollens" looks like a good underlying structure for threats:

- (6) Ernie Blye guarded his Brooklyn tailor shop with a gun and a snarling German shepherd and warned a marauding crowd: "I got ten cans of potash upstairs. I'm goin' upstairs now. I blind you, you come upstairs after me, I blind you." But the looters early on found other targets (Newsweek, July 25, 1977).

Traditional syllogisms can be seen as underlying verbalizations of reasoning:

- (7) How many piano tuners do you think there are in NYC? Well there are three or four in New Haven, which has about 300,000 people. That's one per 100,000. (Collins et al., 1975, p. 405)

Van Dijk (1972) suggests that the conditions for textual coherence are similar to those in formal linear proof; he omits mention of suppositional proof.

In the theory of suppositional proof "an inference is made not from certain previous lines in a proof but rather from (or on the basis of) a certain part of the proof" (Corcoran, 1972, p. 9). Examples of this are "reductio" where "any proof containing a subsidiary proof beginning with P and containing a contradiction may be extended by adding $\neg P$ (not P)" (p. 9) and "conditionalization" "which permits inference of 'if p then q ' on the basis of a subsidiary proof beginning with p and ending with q " (p. 10). Rhetorical questions can function in a manner analogous to the contradiction in reductio proofs. Consider the following:

- (8) This gold figure, 20 ins. high, has normal human proportions, but only four fingers and four toes on hands and feet. The serious scientific explanation? An adding machine! Were the Incas so stupid as to make a whole figure just to represent

a 'four'? Really this is the 'Star God'.
(von Däniken, 1973, caption to Figure 6)

Experiments on discourse recall show that comprehension is influenced by a logical component:

The tendency is to make all detail fit together, so that a story marches to its end without turning off to particular, apparently disconnected and merely decorative material . . . This has been done by the provision of specific links between one part of a story and another, by the introduction of definite reasons, and by the actual transformation of out-of-the-way incidents (Bartlett, 1932, p. 127).

The above examples have glossed over a significant difference between natural language discourse and formal proofs. To claim the similarities required that different expressions be seen as somehow "the same". For example, in (5), the analogy to *modus ponens* required that "He seemed to be disclosing his plans" gave a value "true" to "He would campaign in every primary". Most natural language discourse is dependent on recognizing these kinds of relationships; similarities with formal proofs are less common. For a model of natural language discourse we need to know the "deductive" system that is appropriate for the richness of natural language.

Natural Deduction

Van Dijk (1972) suggests that presupposition and entailment are the terms used for the description of relations between sentences. He distinguishes formal and probabilistic presupposition and entailment. The former are relations definable by the lexicon and semantic rules of the grammar alone. The latter are based on our knowledge of the world. For example, "Do you know when Peter arrived?" has a formal presupposition "Peter arrived.", and "Peter is very ill." has a probabilistic entailment "Peter does not go to work." Most of the logical structure of discourse is based on these uses of world knowledge. In the following narrative there is a mixture of situations and entailments (underlined):

- (9) There his money ran out and he was reduced to sleeping in the open. One day, a man heard him singing and asked him if he knew music and was able to copy it. The invitation not only saved Rousseau from starvation at the time but also showed him how he could earn a little money (Beer, 1972, p. 18).

Intuitively we can sort out the situations and their entailments in (9). Accepting that the entailment structure is in our knowledge of the world, what are the relationships that we are recognizing in the described situation?

Grimes (1975) lists a set of predications between propositions: "alternative" (Your partner may support you or change to another

suit.), "specifically" (He lied, he said he was nowhere near the house at the time.), "evidence" (I saw the ambulance pull up in front of the hotel.), "explanation" (The car won't move unless you release the brake.), "covariance" (His clock stopped. He was late to work.), etc. The separate identity of some of these predicates is open to question; Grimes suggests that explanation may be the same as covariance. Here we are encountering the difficulty in distinguishing a relationship from its function. Halliday and Hasan (1976) have four conjunctive relations, additive, adversative, causal, and temporal. This classification is made using surface connectives, e.g., "and" is additive. However, it is possible for "and" to be the realization of an underlying causal relation: "Fred saw the ghost and screamed."

I want to approach the problem from the observation that utterances can have reference to the external world. Thus a description of events in the world that are perceived as having some unity should constitute a coherent discourse. A description of the actions of a single person--or a contiguous group--should be coherent; the actions of the baby Cromwell in (2), for example. Acts and states in the world are perceived as tied together by space, time, and cause. Discourse based on these would be acceptable. Spatial contiguity gives coherence to the following nursery rhyme:

(10) The king was in the counting house counting out his money.

The queen was in the parlour eating bread and honey.

A description of a sequence of events is also coherent:

(11) After he had lunch, Fred went back to work.

The role of time in giving cohesion to discourse is limited. Of itself it does not produce a very interesting narrative:

(12) After the Battle of Hastings, Drake defeated the Spanish Armada. Later came Waterloo, and later still the Battle of the Little Big Horn.

Relating incidents according to temporal order is a heuristic device; unless there are reasons for the contrary, a dominating discourse organization, incidents are told in temporal order (Langacker, 1969). Spatial organization is similarly not a general way to organize discourse. Space and time are mainly restricted to scene setting roles. Cause may be subdivided into "reason", "result", and "purpose" (Halliday & Hasan, 1976), all of which are cohesive:

(13) He turned on the air-conditioner to get cool.

A fuller discussion is given in Phillips (1975). Using these relationships we can construct a discourse, such as the following, overheard on a bus:

(14) It looks as though we might have some rain. That's not very pleasant in the city. It is good for the

farmers. If they don't get some rain, food prices will go up. Hasn't the price of gas been going up recently? What does it cost these days? That's about what my son said he was paying. My son's in college . . .

This is a very rambling monologue. Lakoff (1972) calls such a discourse "connected" but not "structured" and van Dijk (1972) uses the term "linear", as opposed to "global", coherence.

The aspect of textual organization that is not captured by a "deductive" view of text construction is the imposition of an "attitude" by the author, which Mabley (1972) calls the "theme". The same events can be slanted in different ways to portray different attitudes; one person's hero can be another's villain. Lakoff (1972) uses the CFG formalism to attempt to capture the thematic organization. We have already shown that this model is untenable. Nevertheless it is not without insight. There are other studies that have some commonality with this approach yet do not suffer from the formal inaccuracy. Longacre (1968) and Grimes (1975) have abstract schemata that look like proper analyses of trees that would be associated with Lakoff's deep structures, but are not produced by a CFG. In Grimes there is the notion of "standard configurations" which are very high levels of organization; they are templates and "what we want to say is adapted until it fits one of these available forms" (p. 238). Examples are "plot" (similar to Propp's functions), "star", which is a pattern having "a central point, the conclusion, to which a number of other points contribute more or less independently of each other" (p. 245), and "persuasion" where there are a number of points that "contribute logically to a conclusion that generally involves some kind of alteration of the behavior of the hearer" (p. 245). Longacre (1968) uses various schemata to describe the structure of some genres of Filipino languages. The schema for "narrative" is composed of the following tagmemes:

±APERTURE ±EPISODE ±DENOUEMENT ±ANTI-DENOUEMENT ±CLOSURE ±FINIS

where APERTURE give the spatio-temporal setting and introduces the principal characters, CLOSURE is a final commentary, "They lived happily ever after", and the other tagmemes have as exponents almost any paragraph or even an embedded discourse.

The thematic organization can be characterized by schemata of abstract terms. A discourse has global coherence if it is a realization of an abstract schemata. The evidence of Propp (1968) and Longacre (1968) gives credence to the belief that there are basic patterns for different kinds of discourse. A recursive process of "decomposition" (Phillips, 1975) can rewrite elements of a schema in progressively more concrete terms. This process is qualitatively more complex than string rewriting in a CFG. Any stage in the rewriting can be subject to linguistic encoding to produce the surface form of discourse.

It is possible to build discourse explicitly around decomposition:

- (15) Logic has often been defined as the science that deals with the laws of thought. (Reichenbach, 1966, p. 1)

with the rest of the text being built around this theme. Or the specific statements may be followed by the abstract summary (this process is called "abstraction"):

- (16) Palaiphatos supposes the tale of the shape and parentage of the Centaurs to have arisen from a remark that "the Bullstickers from Nephele (ἐκ Νεφέλης; the phrase might be taken to mean 'born of the Cloud') are raiding", and the supposedly novel sight of horses with men on their backs . . . this . . . "rationalism" (Rose, 1959, p. 5).

Some of the categories of "lexical cohesion" in Halliday and Hasan (1976) serve the same purpose, for example, "idea" in "Henry seems convinced there's money in dairy farming. I don't know what gave him that idea." (p. 275)

A case can be made for two kinds of decomposition. So far I have implied a declarative view; there is a static relationship between the term and its definition. This is acceptable for lexemes like "logic", but it is not a satisfactory view of "rationalism". "Rationalism" is better thought of as being the name of a "procedure", like a computer program. The point may be clearer if I first use the definition of "squareroot" as an analogy. One cannot realistically have a declarative relation for this concept; this would be to have every number we know linked to its square root. Apart from a few rote-learned values, it seems more consistent with behaviour to have some routine for calculating the value--for most of us based on multiplying and adjusting the approximation. Analogously the definition of "rationalism" seems better taken as "being in accord with what one believes" and is a procedure for carrying out this comparison. The concept of "analogy" that I have just used is also clearly another concept that must be a procedure. Metaphor in general is so dependent on the context that it cannot be declarative, it is a judgement made on functionally relevant properties as in

- (17) Is the Chaco cattle country? . . . It's like western Texas, so in some sense I guess it's cattle country. (Collins et al., 1975, p. 404)

I would claim that the schemata for the global organization of discourse are both procedural and declarative in nature.

Strictly abstraction and decomposition relate a definiendum to a definiens in categorical terms. A discourse can contain specific exemplars of the categorical descriptions. It is often found that abstraction/decomposition is combined with "exemplification". In (18) we find the structure definiendum--definiens--example

- (18) The chief characteristic of the protagonist is the desire, usually intense, to achieve a certain

goal . . . Antigone is determined . . . that her brother shall be buried with appropriate funeral rites . . . Othello . . . desperately needs to believe the faithfulness of his beloved wife (Mabley, 1972, pp. 8-9).

The devices that have been presented so far explain the logical and thematic organization of discourse, but there is evidence that another kind of knowledge is used. This is knowledge of the knowledge of others. Consider

- (19) over Oliver's obsequies the lack of regard paid to the coffin was so marked as to further support the theory that it was in fact empty The embalming process . . . had evidently gone wrong Dr. Bate gave a clear description of the autopsy performed on Cromwell's body with its supporating spleen The corpse was embalmed . . . "yet the filth broke through" So wrote Bate . . . "it was prudent to bury him immediately which was done in as private a manner as possible." . . . The Venetian Ambassador referred also to "his actual body being buried many weeks ago." (Fraser, 1973, p. 681)

Some of the facts are attributed to Dr. Bate and some to the Venetian Ambassador. The report of Dr. Bate is constructed using a causal sequence of events and states; the Ambassador merely gives the same conclusion without reason. Overall this is an example of the "star" predicate of Grimes (1975), in that the two points have the same conclusion.

Summary

I have tried to indicate the mechanisms that are used in constructing discourse without considering the function or genre for which each may be appropriate. A distinction was made between thematic and logical organization. The former uses the processes of abstraction, decomposition, and exemplification. Logical organization has a component based on knowledge of space, time, and cause. Some formal structures, syllogism, for example, are also found. Knowledge from other sources can also be introduced into discourse. This knowledge may be organized in its own right according to any of the principles outlined above.

Sometimes the organization is expressed, otherwise it has to be inferred by the hearer on the assumption that the discourse is coherent and organization is present. The inferencing is constrained to the above mechanisms.

References

- Bartlett, F. C. Remembering. Cambridge: Cambridge University Press, 1932.
- Beer, G. de. Jean-Jaques Rousseau and his World. New York: Putnam, 1972.
- Collins, A., Warnock, E., Aiello, N., and Miller, M. Reasoning from incomplete knowledge. In D. G. Bobrow and A. Collins (Eds.), Representation and Understanding. New York: Academic Press, 1975.
- Corcoran, J. Discourse grammars and the structure of mathematical reasoning: III. Two theories of proof. J. Structural Learning 3, 1973, 1-24.
- Däniken, E. von. The Gold of the Gods. New York: Putnam, 1973.
- Dijk, T. A. van. Some Aspects of Text Grammar: A Study in Theoretical Linguistics and Poetics. The Hague: Mouton, 1972.
- Fraser, A. Cromwell: The Lord Protector. New York: Knopf, 1973.
- Grimes, J. E. The Thread of Discourse. The Hague: Mouton, 1975.
- Keenan, E. L. A logical base for a transformational grammar of English. Unpublished doctoral dissertation, University of Pennsylvania, 1970.
- Halliday, M. A. K., and Hasan, R. Cohesion in English. London: Longman, 1976.
- Hopcroft, J. E., and Ullman, J. D. Formal Languages and their Relation to Automata. Reading, Mass.: Addison-Wesley, 1969.
- Lakoff, G. Structural complexity in fairy tales. The Study of Man 1, 1972, 128-150.
- Langacker, R. W. On pronominalization and the chain of command. In D. A. Reibel and S. A. Schane (Eds.), Modern Studies in English: Readings in Transformational Grammar. Englewood Cliffs, NJ: Prentice-Hall, 1969.
- Longacre, R. E. Philippine Languages: Discourse, Paragraph, and Sentence Structure. Santa Ana, CA: Summer Institute of Linguistics, 1968.
- Mabley, E. Dramatic Construction. Philadelphia: Chilton, 1972.
- Phillips, B. Topic analysis. Unpublished doctoral dissertation, State University of New York at Buffalo, 1975.
- Propp, V. Morphology of the Folktale. Austin: The University of Texas Press, 1968.
- Reichenbach, H. Elements of Symbolic Logic. New York: The Free Press, 1966.
- Rose, H. J. A Handbook of Greek Mythology. New York: Dutton, 1958.
- Rumelhart, D. E. Understanding and summarizing brief stories. In D. LaBerge and J. Samuels (Eds.), Basic Processes in Reading: Perception and Comprehension. Hillsdale, NJ: Lawrence Erlbaum Associates, 1976.

THE GRAMMAR OF THE BASQUE PROVERB

Terence H. Wilbur
University of California, Los Angeles

In this paper I wish to investigate the linguistic means that change an acute observation into a proverb. These are the means employed by a particular language, Basque. I shall not be able, except by indirection, to deal with the essence of the proverb or with the problems of its universal occurrence. I wish to point out that in a specific tradition certain specific grammatical features distinguish proverbs from ordinary sentences. In other words, proverbs have a special surface grammar.

One definition of the proverb is a proverb itself:

1. *The wisdom of many, but the wit of one.*

This proverb emphasizes two linguistic features of the genre, the transmission of the proverb and its creation. The latter feature points out that the proverb is created at a specific moment in time by a witty person. This paper is concerned with the grammatical means that the wit has at his disposal for the summation of his witticism. The means by which this witticism in its specific grammatical form is taken into the tradition and is pulled out upon the appropriate occasion as received wisdom cannot be discussed within the framework of this paper. It must be said, however, that the proverb is transmitted as a lexicalized sentence. This fact is expressed clearly by the classical definition, *id quod pro verbo dicitur* (which is attributed by some to Aristotle).

This study lays emphasis upon the creation of the proverb, not upon its content, but upon the grammatical machinery used to make it. Basque makes available to the originating wit devices that say in effect: "This is a proverb." In this case we may not speak of the limits of grammaticality or of deliberately deviant grammar. The genre has its own quite regular grammar. To be sure, these grammatical means are somehow related to those devices that a linguistic tradition puts at the disposal of the versifier. It must be noted that verse tends toward expansiveness while the proverb tends toward extreme brevity. We might even characterize proverb grammar as an abbreviation grammar. The utterance is systematically stripped down and compacted. The most famous of Basque proverbs is an example of this extreme compaction:

2. *Atsherri, otsherri.* (0 49)¹

"Stranger-land, wolf-land."

It is possible to expand this utterance into a complete and normal sentence:

3. *Atsearen herria otsoaren herria da.*

"The land of the stranger is the land of the wolf."

One of the problems we must address ourselves to is whether the compacted form is arrived at by a system of deletions or whether the grammar immediately generates the proverb. For the moment, I shall naively assume that the proverb is a stripped-down sentence.

I use as my source of examples Julio de Urquijo's edition of *Los Refranes y Sentencias de 1596*, originally published in the *Revista Internacional de Los Estudios Vascos*, volumes 5(1911) through 24(1933) and subsequently published as a whole in the *Collección Aunamendi* 1964. A cursory inspection of the contents of this collection of proverbs reveals the remarkable repetition of the pattern: NP, NP, with the absence of an inflected verb. Logically the two terms, the NP's, are connected by an implication that says, "If A, then B." We could, therefore, paraphrase proverb 2 above as: "If you are in a foreign country, then you are subject to attack by the wolfish inhabitants."

Let us examine a proverb that occurs in two forms.

4. *Kantari txarra beti kantetan dago.* (U 19)

"The bad singer always keeps on singing."

This is a normal and unremarkable Basque sentence. The properly proverbial form appears as:

5. *Kantari geistoak onon.*

"The bad singer (has) persistence."

Grammatically the first NP in this implication is in the ergative case. This indicates that a transitive verb, probably *badu*, has been suppressed. The greater number of implicational proverbs fall into the pattern: NP¹ (is) NP", or, NP¹ (has) NP". However, in other ergative constructions different verbs have obviously been suppressed.

6. *Erle ioan-nahiak, ez esti, ez bresca.* (O 146)

"The bee that wants to leave (the hive makes) neither honey nor honeycomb."

In this case, the verb meaning to make or to produce is omitted. We might say that this is a classical case of an understood verb. Except for the ergative inflection, the surface form remains the same as the equational forms in the following proverbs:

7. *Adiskidok etsai.* (U 130)

"Friends, enemies."

8. *Andra ederra etxean gerla.* (U 131)

"Beautiful woman, war in the house."

Whenever, in proverbs of this type, the verb or verbal idea must be realized, the verb occurs in a special non-inflected form.

9. *Otsoa non aipa, han gerta.* (O 385)

"Wherever (you) mention the wolf, there (he will) appear."

This would seem to be the stripped-down version of a possible sentence:

10. *Otsoa non aipatu duzun, han gertatuko da.*

In terms of verb morphology, *aipa* and *gerta* are special participial forms that occur only in conditional, supposition, potential, and other non-real inflected forms. In addition, the form is used in rather indefinite forms of the imperative where no particular person is addressed:

11. *Zuhauk ikus.*

"See for yourself!"
and in even more indefinite questions:

12. *Nork erran?*

"Who (is to) say?"

Classical Basque grammarians have typically described this form, the so-called radical infinitive, in the same terms as Pierre Lafitte (1963: 200): "*L'infinitive radical est la forme qui exprime l'idée verbale de la façon la plus indéfinie.*" Thus, by the use of this peculiar form of the verb, which is in fact a nominalized form, the observation is made very general and non-temporal. In the process of "proverbialization" the observation is made general and lifted into the realm of eternal verities by the suppression of the finite verb and, whenever possible, by the complete obliteration of any verbal forms. We must note, too, that proverbs by use of participial forms, nominal in nature, turn ordinary sentences into totally nominalized strings. It is very important to take into consideration that the indefinite imperatives and indefinite mentioned above have the same grammatical shape as the proverb.

I have gingerly avoided any definition of the nature of the proverb in its complete essence. For Basque, I can dare give such a definition in purely grammatical terms. In Urquijo's edition of the proverbs of 1596, of the 163 proverbs quoted and analysed in the first volume, one hundred are completely of the grammatical form I have described. Sixty-three of these so-called proverbs, according to my definition, must be relegated to the classification of wise sayings or philosophical insights -- which, of course, by no means, damages their validity--. We must not be drawn into the question of the point where grammar leaves off and rhetoric begins.

I claim that the speaker of Basque has at his disposal a special kind of syntax just for proverbs. Characteristic of this very special syntax is the non-realization of finite forms of the verb that are otherwise absolutely obligatory in other kinds of sentences. (Incidentally, we are permitted at this point to emphasize the inadequacy of sentence-grammar.) The trigger for the occurrence of sentences of this type would be a kind of modality that might be labeled "eternal verity." We are permitted in this case to add to the very long list of Basque tenses and moods a new one, whose realization is zero.

I must point out that I have treated the creation of proverbial utterances as a matter of syntax. One fact stands in the way of this point of view. Once it has found acceptance, like a newly formed compound noun, the proverb is treated rhetorically and transmitted linguistically as a part of the lexicon. Therefore, if one adheres to the lexical hypothesis, comparison with the morphological rules for the formation of compound nouns is tempting. It would seem then that the formation of proverbs lies in that shadowy region between syntax and morphology. If we have obscure places in our theories, our theories must be lacking in adequacy of any sort.

In this small presentation, I have endeavored to be suggestive rather than definitive. Certainly a genre of such universal distribution such as the proverb deserves a thorough linguistic treatment. To my colleagues in anthropology, literary history, and folklore this may seem to be a little bit presumptuous. However, the linguistic study of this genre will certainly be rewarding.

13. *Gitxia, gitxia asko egin doa.* (U 61).

"Little by little a great deal gets done."

FOOTNOTES

1. The numeration refers to the order of proverbs in the collections of Oihenart (1847) and Urquijo (1964).

REFERENCES

- Lafitte, Pierre. 1963. *Grammaire Basque (Navarro-Labourdin littéraire)*. Bayonne: Amis du Musée Basque.
- Oihenart, Arnaut de. 1657 (1847). *Les Proverbes basques recueillis par le Sr. d'Oihenart, plus des poésies basques du mesme auteur*. Paris. 2d. edition. 1847. Bordeaux: Bibliotheca Colbertina.
- Taylor, Archer. 1931. *The Proverb*. Cambridge, Mass.: Harvard University Press.
- Urquijo, Julio de. 1964. *Refranes Vasco. Los refranes y sentencias de 1596*. San Sebastian (Guipúzcoa): Editorial Aunamendi.

BEYOND THE FRAGMENT: USING STRATIFICATIONAL AND
TAGMEMIC THEORY IN AN ANALYSIS OF PASCAL'S PENSÉES

Deborah Tyma
University of Michigan

0. Introduction.* With the increasing interest in studying samples of linguistic data larger than the sentence, the gap between linguistics and literary criticism appears to be shrinking. Because literature is nothing if it is not first the use of language, the critic may benefit from textual insights offered by linguistic theory. Both stratificational and tagmemic theory can contribute, especially in the clarifications and metaphors both theories offer on text structure and on the relation of form and content.

The problem of the relation between the content of a work and its form¹ has particular relevance to a consideration of Pascal's Pensées. Like other unfinished works, the Pensées, the thoughts Pascal collected in preparation for writing an apology of the Christian religion, have tempted scholars of later centuries to finish the author's work, to establish the 'true plan' which the Apologie would eventually have followed had Pascal lived to complete it. The form in which he actually left the Pensées, fragments filled with antitheses and paradoxes, is thus rejected as irrelevant to his message.

But the author himself required that style and organisation reflect meaning: '[L]'éloquence] consiste donc dans une correspondance qu'on tâche d'établir entre ... les pensées et les expressions dont on se sert' (fr. 16). Figures and expressions are of no use to Pascal unless they actually reflect and correspond with the content of the text: 'Ceux qui font les antithèses en forçant les mots sont comme ceux qui font de fausses fenêtres pour la symétrie:² leur règle n'est pas de parler juste, mais de faire des figures justes' (fr. 27). Given these personal standards of eloquence, some critics, such as Goldmann (1955), defend the fragmentary organisation of the work as the only one that could adequately communicate Pascal's message; the content, in effect, required the style and organisation that the author used.

Both tagmemic and stratificational theory deal with the problem of reconciling expression and meaning. To demonstrate how each theory can contribute to a fuller appreciation of Pascal's work, I will first briefly sketch the ideas presented in the Pensées, then propose a relational network describing the connection between Pascal's thoughts and his expression, and third, discuss the application of Pike's three perspectives on language to the Pensées. Finally, I wish briefly to consider some implications, both for literary critics and for linguists, of using such linguistic insights in a close reading of a text.

1. Meaning. In Pascal's view, paradox is the chief characteristic of man's life on earth. His world is one of impossible and unceasing contradictions: the contrary man is constantly torn between opposite extremes: 'ayant [la raison] et [les passions], il ne peut être sans guerre, ne pouvant avoir la paix avec l'un qu'ayant guerre avec l'autre: ainsi il est toujours divisé, et contraire à lui-même' (fr. 412). With no absolute values in his contradictory soul, man can find none in his paradoxical world: 'rien, suivant la seule raison, n'est juste de soi, tout branle avec le temps' (fr. 294). Even man's perception of God wavers; Pascal's God is one who is always present, yet always absent: 'Si je ne voyais rien [dans la nature] qui marquât une Divinité, je me déterminerais à la négative; si je voyais partout les marques d'un Créateur, je reposerais en paix dans la foi. Mais, voyant trop pour nier et trop peu pour m'assurer, je suis dans un état à plaindre' (fr. 229). Faced with such contradictory evidence, Pascal cannot introduce an assertion without also introducing its contradiction: 'Incompréhensible que Dieu soit, et incompréhensible qu'il ne soit pas' (fr. 230).

The double bind of simultaneously admitting two opposite propositions leads to Pascal's final paradox. Although absolute values cannot and do not exist in this world, man must still search for such absolutes. This requirement for the search for the impossible absolute, advanced in spite of the denial of its existence, renders Pascal's thoughts tragic, because it is a demand for totality. In the world of contradictions any totality must encompass all the opposing assertions. A synthesis of contradictions thus becomes, for Pascal, the only absolute value possible: 'Pour entendre le sens d'un auteur, il faut accorder tous les passages contraires ... Tout auteur a un sens auquel tous les passages contraires s'accordent, ou il n'a point de sens du tout' (fr. 684). For Pascal, synthesis is the meaning man can derive from his paradoxical existence, the meaning of his experience in the world in all its contradictory aspects.

2. The stratificational contribution. In the light of such a requirement for synthesis and totality, the critic may expect to find in the Pensées a unity between thoughts and expressions. In Figure 1,³ the realisational nature of the relationship between form and content in Pascal's work is graphically demonstrated.

Following the realisational portion of the network, one may discern in the Pensées three levels of form--the antithesis, the fragment, and the work--and a set of conceptual correlations: the contradictory man, the paradoxical world, and the hidden God. The conceptual correlations of Pascal's view of his world comprise his subject or content, which is realised by 924 fragments, only three of which are here represented with nodes. Finally, each of the fragments is itself realised by one or more antitheses and paradoxes.

The tactic portion of the network demonstrates how the fragmentary organisation of the Pensées works, thereby allowing the paradoxical nature of the fragment to be fully understood. Because of their fragmentary form, the Pensées may be regarded

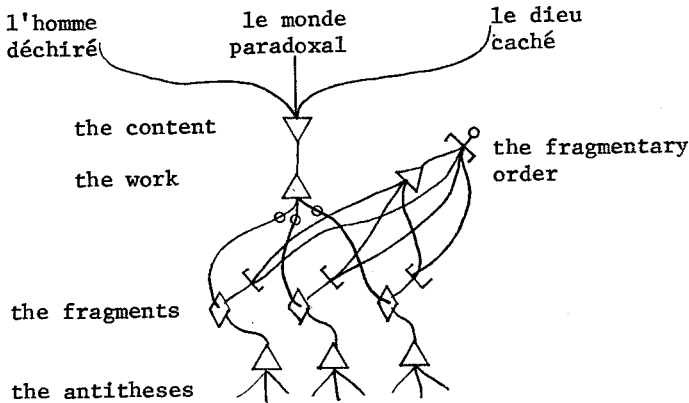


Figure 1. A relational network describing the structure of the Pensées.

either as a multitude of separate thoughts or as a coherent unity. Similarly, while each fragment is a part of the work, composing with all the other fragments the entire work, a single fragment may alone realise the content. For example, fragment 272 summarises the basic contradiction found in man, his world, and his God: 'Il n'y a rien de si conforme à la raison que ce désaveu de la raison.' While this short sentence expresses the subject of the entire Pensées, it is at the same time contained by the work. Therefore, the tactics, with its downward 'or', describes two ways for the fragments to realise the work: either all the fragments together, represented by the 'and', may realise the content, or each individual fragment, like fragment 272 above, may itself realise the entire Pensées. Demonstrating two possible realisational combinations, the tactics allows the critic to see the two contradictory aspects of the paradoxical fragment: it is the whole, and a part of the whole, at once.

Taken together, the tactic and realisational portions of the network show that the fragmentary order, the paradoxical fragment, and the antitheses are uniquely well-suited to express Pascal's description of the contradictory world. To the critic, stratificational theory thus contributes two metaphors: while the tactic metaphor allows a fuller understanding of the paradoxical fragment; the realisational metaphor demonstrates the union between the paradoxical elements of form Pascal used and his message of contradiction and synthesis.

3. The tagmemic contribution. A problem arises, though, in attempting to apply these metaphors too strictly. While the critic must deal with the fragments as separate sections of the text, the existence of units within the system is irrelevant in stratificational theory.⁴ In order to treat the fragments as units, therefore, the critic must depart from the stratificational metaphor. But conceding the unitary status of the fragment appears to bring one dangerously close to destroying the unity of the text. For a resolution to this problem, the critic may turn to a

consideration of tagmemic theory.

In 'Beyond the sentence', Pike (1964) demonstrated the application of his twelve tagmemic principles to the analysis of a poem. It is my contention that all twelve principles can successfully be applied to a study of the Pensées, but, since the present concern lies mainly with the relation of the fragments to the entire work, the notions of the three complementary perspectives on language will be most helpful here.

Following Pike (1959), in which the three perspectives are described in detail, one may say that the traditional view of the Pensées takes a particle perspective: the work is one large unit made up of 924 smaller particles or fragments. In turn, each of these particles is composed of sentences, phrases, and words, which themselves may be broken down into smaller units. Such a view is indeed valid, but Pascal urges the critic on: 'Mais les parties du monde ont toutes un tel rapport et un tel enchaînement l'une avec l'autre, que je crois impossible de connaître l'une sans l'autre et sans le tout' (fr. 72). Looking at the whole, the observer may see the Pensées as a wave composite (Pike 1964), with the fragments merging together into liasses, the sections of the work dealing with different aspects of Pascal's main concern. Seen in this view, the liasses are wavelets fusing together into the behavioural wave of communicating the notion of the paradoxical world. Just as, in the phonological hierarchy, late-appearing sounds may be anticipated earlier in a sequence, thoughts of a later liasse may be anticipated in earlier sections. Thus the work may not be as easily segmentable as it appears; a wave perspective becomes necessary. The perspective chosen by the observer thus allows a perception of the Pensées either as a multitude of parts or as a unity; neither conception is complete without the other. As particle and wave, multitude and unity at once, the work is highly paradoxical. Thinking of the Pensées in two such different fashions allows the observer-critic to see that the work itself is the ultimate antithesis, reuniting two completely opposite perspectives.

Of the twelve tagmemic principles (Pike 1964:193), the third complementary perspective, the field view of language, is one of the most interesting for the literary critic. This principle requires an understanding of the total system of a language sample, as well as of its context. If a particle view shows language as static, and if a wave view shows the dynamic aspect, then a field perspective allows one to see language as a functional system wherein 'no parts occur apart from their function in the total whole' (Pike 1959:129). Thus, in a matrix set up by interlocking universes of discourse, the function of each element of the system becomes clear. Figure 2 shows such a matrix constructed for the Pensées.

The dimensions that control this abbreviated matrix are basic: the topics with which Pascal concerned himself and his modes of thought. The intersection of these universes of discourse sets up the field in which the fillers of the cells find their functional relationships. Each column of cells represents

	man	God	values	Deity
thesis	ange	exists	no absolutes exist	God
antithesis	bête	doesn't exist	must search for absolutes	man
synthesis	homme	the wager	totality	Jesus

Figure 2. An abbreviated matrix representing the field perspective of the *Pensées*.

a process of thesis-antithesis-synthesis, which, as Goldmann (1955) has noted, is not just a figure of expression for Pascal, but 'a primary fact of existence'. As opposites, the two higher cells of each column form a dialectic in which the worldly man finds himself trapped. The only resolution, as mentioned above (section 1), is the reunion or synthesis of the two opposites. Man is thus divided between two contrary natures; he has the characteristics of both the beasts and the angels, yet he is neither: he is a man. The second column shows Pascal's response to the problem of the existence of God: he wagers that God does exist, which is much less risky than betting that he doesn't. The wager takes into account both the thesis and the antithesis; it is their synthesis. The third column describes the double bind, as well as Pascal's demand for the search for totality in the paradoxical world. The last column shows that, for Pascal, Jesus was much more than a prophet; he was both God and man, a synthesis of violently contradictory natures.

A crucial point in matrix theory (Pike 1962) is that the fillers of all cells in the same rank--or in the same column--share one contrastive feature, while one other feature distinguishes each filler from the others. What the fillers of the four cells of the lowest rank have in common is that they are all resolutions to different dialectics. As syntheses, man, the wager, the search for totality, and Jesus fulfill the same function in Pascal's paradoxical world. The matrix, therefore, serves to emphasise the relationships of seemingly disparate elements of the text.

Since the process of thesis-antithesis-synthesis is integral to the author's style, the matrix may be expanded to include literary form:

	man	God	values	Deity	literary form
thesis	ange	exists	no absolutes exist	God	the 'true order'
antithesis	bête	doesn't exist	must search for absolutes	man	no order
synthesis	homme	the wager	totality	Jesus	the fragmentary order

Figure 3. The expanded matrix.

Figure 3 graphically demonstrates the non-arbitrariness of the form of the *Pensées*. Not only does the fragmentary order reflect

man's search for synthesis; it is itself a synthesis, participating in the same functional relationships as the fillers of the other cells in the lowest rank. The use of the fragmentary order is therefore just as vital to the development of Pascal's Apologie as is his discussion of the paradoxical man. Any other organisation would have failed to satisfy Pascal's requirement for correspondence between 'les pensées et les expressions dont on se sert' (fr. 16).

The insights offered by tagmemic theory may therefore be most useful to the pascalian critic. The particle and wave metaphors allow the observer to consider the text from two simultaneous perspectives and thus to appreciate fully the paradox of the work. The use of matrices helps the critic discover the interrelations of all elements of a text and, in the case of the Pensées, clarifies the non-arbitrariness of form.

4. Implications of the stratificational and tagmemic analyses. Both the stratificational and tagmemic analyses of the Pensées have emphasised the unity of form and content. In addition, the stratificational network revealed the paradoxical nature of the fragment, while the particle and wave perspectives showed the paradoxical nature of the complete work. The insights of both theories are thus needed to understand the way in which all aspects of the text, even its style and organisation, fit into a system whose function is the communication of Pascal's message. Finding such an orderly system in the midst of the chaos of the contradictory world evokes a feeling of exhilaration; in such a discovery, the emotional and intellectual responses to literature have coincided.

Linguistic theory can guide the reader to such an experience of coincidence, thereby heightening his appreciation of the fabric and magic of the text. Perhaps, then, the study of aesthetic impact need not be excluded from the linguist's realm. Any metaphor that can give insight into the function of each element of a work in the textual tapestry woven by the author will surely be useful to one considering the artistic aspects of the work. In this direction, the foregoing analyses have attempted to show how a study of the interrelations of style, organisation, and content can be approached from a linguistic perspective, with the ultimate goal of a better understanding of aesthetic impact.

Relational networks have been used before to describe data outside the rather narrow confines of linguistics (see Lockwood 1972 for examples). And one of the major goals of tagmemics is to describe all types of purposive human behaviour (Pike 1956). So there is precedent for the application of the two theories to extra-linguistic data, although the insights they offer have not been extensively employed in a study of aesthetics.⁵ Such an extension of its domain may be found to tax unduly the resources of linguistic theory. But, on the other hand, stretching tagmemics and stratificational theory to their limits by applying them to different aspects of human activity may prove enlightening. The effort of an unfamiliar task could emphasise the differences between the two theories, sharpen the contrastive fea-

tures, and perhaps show areas in which one is more useful than the other. It could also show how stratificational and tagmemic theories complement each other, thereby avoiding a dialectic between the theories and promoting a resolution of their contradictions.

Notes

* A portion of this paper was presented at the Third Michigan State University Mini-conference on Language and Linguistics under the title 'The Unity of form and content in Pascal's *Pensées*: an application of stratificational theory to literary criticism'. I am grateful to Michael S. Koppisch, David G. Lockwood, and A.L. Becker for having read and commented on earlier versions of the current paper. I have benefitted greatly from discussions with Stephen Tyma.

¹ The term 'form' is used in its literary rather than strictly linguistic sense to refer to the surface appearance of the work, including style and organisation. The present usage of the word differs, therefore, from Lamb's in which 'form' refers to underlying rather than surface system.

² Building false windows to achieve a symmetrical outward appearance was at one time a common practise in France; many windows at Versailles, for example, open onto closets, walls, or other inappropriate places.

³ I am indebted to C.W. Johnson and D.G. Lockwood for their helpful comments on Figure 1.

⁴ For a further discussion of this point, see Lockwood (1972), especially page 5.

⁵ The current work of A.L. Becker is most interesting in this regard, including his study of the aesthetics of Javanese shadow theatre (in press) and his tagmemic interpretation of 'The Manor Garden', a poem by Sylvia Plath (personal communication).

References

- Becker, A.L. (In press). Text-building, epistemology and aesthetics in Javanese shadow theatre. In A. Becker and A. Yengoyan (eds.), *The Imagination of Reality*. Ann Arbor: Association for Asian Studies.
- Brend, Ruth M. (ed.). 1972. *Kenneth L. Pike: Selected Writings*. The Hague: Mouton.
- Goldmann, Lucien. 1955. *Le Dieu caché*. Paris: Gallimard.
- Lockwood, David G. 1972. *Introduction to Stratificational Linguistics*. New York: Harcourt, Brace, Jovanovich.
- Pascal, Blaise. 1964. *Pensées*. Ch.-M. des Granges (ed.). Paris: Editions Garnier Frères.
- Pike, K.L. 1956. Towards a theory of the structure of human behaviour. In Brend (ed.), 1972. *Selected Writings of Kenneth L. Pike*. The Hague: Mouton.
- _____. 1959. Language as particle, wave, and field. *Texas Quarterly* 2:2, 37-54. (Also in Brend, 129-143.)
- _____. 1962. Dimensions of grammatical constructions. *Language* 38:221-244. (Also in Brend, 160-185.)
- _____. 1964. Beyond the sentence. *Journal of the Conference on College Composition and Communication* 15:129-135. (Also in Brend, 192-199.)

GRAMMATICAL VERSUS REFERENTIAL STRUCTURE
ILLUSTRATED IN ENGLISH DEFINITION
Doris Lander
Summer Institute of Linguistics

1. INTRODUCTION

When we look at a discourse, we may view it from one of (at least) three perspectives. We may look at the patterning and interrelationships of sounds; we may look at the patterning and interrelationships of grammatical classes; or at the content of the discourse--that is, how linguistic reference is made to entities and events in the real world (or a hypothetical world that might be created in the discourse) as they are perceived by an observer. The last includes linguistic reference to events and sequences of events, participants, identities, concepts, and in particularly non-narrative discourse types, certain aspects of logical structure. I will refer to these three perspectives on language as components--phonetic-phonological, grammatical, and referential.

Suppose we are in a crowded room and overhear Jane asking her older brother Bill, "Do you know what a vinculum is?" We may look at this short discourse from one of these three just-mentioned perspectives. We can determine what are its phonetic characteristics and phonemic units: final rising intonation; stress on the first syllable of vinculum; an initial apical voiced stop; etc. Secondly, we can look at its syntactic or grammatical structure. As a whole it is a question, as demonstrated (partly) by word order where the verbal auxiliary do precedes the subject you. We also may note that there is agreement of person between subject and verb in English by contrasting this clause with "Does he know what a vinculum is?" where does correlates with third person he, and do correlates with second person you; and so on. Thirdly, we can look at the way in which this utterance refers to the real world. In the social situation, Jane is addressing Bill directly and consequently you is the appropriate way for her to refer linguistically to the real-world person Bill. We may surmise that "a vinculum" is Jane's way of referring linguistically to some (apparently) real-world concept of which either she does not know the identifying characteristics, or else is just interested to see whether Bill does.

This is intended to be just a very brief illustration of the three different perspectives. In the rest of this paper I will be ignoring the phonetic-phonological component and my purpose is (1) to discuss some of the differences between grammatical and referential structure; (2) to discuss some referential characteristics of English definition types; and (3) to briefly show how one referential structure may be encoded in various grammatical structures.

Before looking at definition specifically, however, I will briefly discuss four essential features of any linguistic unit, as emphasized in tagmemic theory: slot, class, role (or function), and cohesion. When an item or entity occurs within some larger context, it is assumed that it occurs in some "position" within that context, whether the position be determined relative to sequence in time (e.g. before, after); linear or spatial ordering (preceeding, following, next to); logical configuration; or other. The position where the item or entity occurs may be thought of as a slot. In the theory of tagmemics as developed by Kenneth L. Pike, just two slot types are generally distinguished: nucleus and margin. Nucleus may be thought of as the most prominent, important, or characteristic part of a structure; the nucleus tends to be "structure defining" in that normally, it is what makes a structure that structure and not some other (Jones 1977:3,4). Margins are less prominent and normally less crucial in determination of particular structures. Depending on the context, margins may sometimes be absent.

A class may be simply thought of as the set of all entities that are found to occur in a particular slot. In other words, an element of a class is a "filler" for a slot. A grammatical class consists of types of syntactic entities or constructions, e.g. the classes of noun phrases, transitive clauses, expository paragraphs, etc. A referential class consists of the linguistic references to entities or items occurring in some socio-cultural or conceptual real-world slot. For example, a referential class might consist of references to the set of persons who attended Linguistics 612 at the University of Michigan during 1976. Or it might consist of all the televised speech events performed by Gerald Ford and Jimmy Carter during the 1976 presidential campaign as they debated with each other. For each of these very different types of classes, the surrounding context in which they occur is essential in determining which items are indeed members of a common set. In the following discussion, I will use the shorter term "(referential) identity" (or "set," "component," etc.), rather than the longer but technically correct "linguistic reference to real-world identity" (or "set," "component," etc.). It should be kept in mind that the former is just a short-hand way of saying the latter.

Role or function is an item's purpose in relationship to other elements in the larger context. For example, The boy in The boy hit the dog has the grammatical role of actor. Referentially his role may be something more like hero or villain, depending upon the situational context. It is possible that different observers of an item in its context will perceive different roles for that item, particularly with regard to referential structure.

Cohesion refers to governing relationships; that is, what governs the presence or absence of a particular unit or feature of a unit; or how that unit governs the presence or absence of some other unit or feature of a unit. At the highest level this involves the "universe of discourse" within which one is speaking or writing. For example, the context of mathematics might dictate that I define things in a certain prescribed way, versus the

context of anatomy, as I will discuss later in this paper.

These four features of any linguistic unit may be summarized in the following notation (Pike and Pike 1977):

SLOT	CLASS
FUNCTION	COHESION

2. GRAMMATICAL VS. REFERENTIAL STRUCTURE, ILLUSTRATED IN ENGLISH DEFINITION.

I will now turn to look at some features of grammar versus reference, as illustrated in English definition types. The following example (taken from Trimble 1976:8) is one instance of definition:

- (1) A triode is a vacuum tube which contains (containing) three elements: a cathode, an anode, and a grid.

According to Trimble's analysis, this is an instance of a "formal" definition, which consists of:

1. Term--the concept being defined: this is the naming function;
2. Class--the set of concepts of which the given concept is a member: this is the classifying (locating) function; and
3. Difference(s)--the basic difference(s) between the term being defined and other members of the class (other subsets of the set).

For the example above, A triode is the term (or referential NAME assigned to the real-world concept or identity); a vacuum tube is the CLASS (or referential name for the larger including set of which triodes are members); and the differentiating (CONTRASTIVE) components or features are given in the relative clause which contains (containing) three elements: a cathode, an anode, and a grid. This sets it off as being different from a diode, for example.

Modifying slightly the terminology Trimble uses, the general referential structure¹ of a formal definition can be represented in the following way (returning later to discuss features of slot and cohesion):

and cohesion):		Set (of identities)	
(2)	Identity _i	i	
Naming		Classifying/ locating	
		Contrastive components of Identity _i	
Contrasting			

In Sec. 1 I partly characterized the referential component as being the perspective from which one looks at how linguistic reference is made to (real or hypothetical) identities, entities, or concepts. Consequently, it follows directly that the structure given in (2) is referential--not grammatical--in that it characterizes how we linguistically define real-world identities (such as triodes).

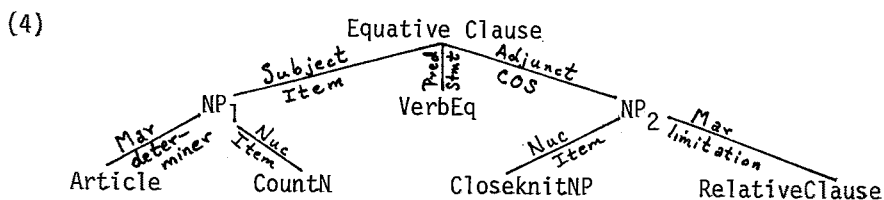
We can look again at the data in (1) from which (2) was in part derived, but from a different perspective, and abstract the following structure.

(3)	Subject	NP	Predicate	VerbEq	Adjunct	NP
	Item		Statement	Equative	Characteristic of Subject	

Here we have three constituents. First, the subject with role of item, filled by an NP. Secondly, the predicate whose role is to make a statement, filled by an equative verb. And thirdly, the adjunct whose role is to give some characteristic(s) of the subject, filled by another NP.

One of the ways that the referential structure (2) differs from the grammatical structure (3) is by the nature of the filler classes.² In (3) we have syntactic entities or constructions: NP and verb. They are identifiable as syntactic partly by their occurrence in "positions" or slots which are appropriate only for syntactic constructions--such as a nuclear subject, predicate, or adjunct slot. For example, it is not appropriate to say that a proper NP debated with Jimmy Carter in the 1976 presidential campaign, because the linguistic description here refers to a real-world event (or sequence of events). Rather, a particular real-world identity debated with Jimmy Carter, and the way we refer linguistically to that identity is by using the lexical item Gerald Ford. Conversely, it is not technically appropriate to say that Gerald Ford is the subject of the clause, Gerald Ford debated with Jimmy Carter in the 1976 presidential campaign. Rather, a proper NP is the subject, and the particular lexical item manifesting the proper NP is Gerald Ford.

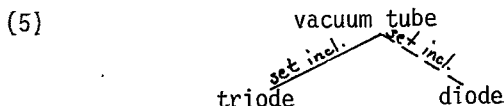
In addition to the types of classes found in grammar versus reference, these two components of language also differ in their hierarchical organization and the types of relationships found there. To illustrate this, I will present the structure of (1) schematically from both perspectives. The following presents the grammatical structure of this particular equative clause type.



Here the various slots of subject, predicate, and adjunct are manifested by grammatical constructions--NPs and a verb. The NPs have further internal complexity. NP₁, for example, has a margin slot, with the role of determiner, filled by an article; and a nucleus slot, with role of item, filled by a count noun. In this particular case the instantiations of the classes of article and count noun are a and triode respectively.

The type of hierarchical organization here is strictly part-whole: an article and a count noun are parts of the whole noun phrase; the two noun phrases and verb are parts of the whole equative clause. The hierarchy is not taxonomic, in that the article and count noun are not types of a noun phrase.

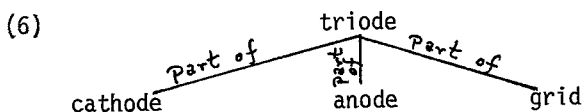
In turning to the referential structure, however, we find that the organization of entities in relationship to one another is more complex--there are both taxonomic and part-whole relationships.



In (5) I have shown by solid lines the information which is actually present in (1), and filled in some other bits of information in order to more completely show the nature of the relationships involved. Triodes and diodes are types of vacuum tubes--they are not parts of vacuum tubes. The relationship here is one of set inclusion: the specific (triode) is a member of the more generic set (vacuum tube).

"Set inclusion" is a relation resulting from specification of contrastive features or components. With regard to the definition in (1), we find that cathode, anode, and grid are the contrastive components which serve to both identify a particular vacuum tube as a triode, and to differentiate it from a diode, which consists only of an anode and a cathode. It is specification of these contrastive-identificational components which serves to break up a set into its constituent members. We may simply say "set inclusion" to label the relation between two entities provided we understand that that implies that there are contrastive features which serve to differentiate each member of the set from each other member. If we perceive a set to be a one-member set, we have reached a threshold level where further specification of contrastive features has become meaningless to us.

The definition in (1) also gives the following information:



Here, a cathode, anode and grid are the significant parts of the whole--they are not further subtypes of a triode. Thus, we see that hierarchical relationships³ between referential entities can be either set inclusion (taxonomic) or part-whole.

3. SOME REFERENTIAL CHARACTERISTICS OF ENGLISH DEFINITION TYPES

In addition to "formal" definition presented earlier (Sec. 2), Trimble discusses features of various other definition types. In this section, I will discuss them from a referential perspective, note some of their differences, and note what all varieties of

In order to be effective in terms of speaker-hearer relations, conceptually a definition must link unknown to known.⁴ The speaker relies on information he assumes the hearer already knows, and tries to create a new referential configuration in his mind by linking that old, known information in some particular way with a new label. Thus, one major purpose of a definition is to present some referential equivalent to the newly named identity. In all of the formulations of definition to follow, the consistently present constituent is one with a naming function which labels the identity presumably unknown to the hearer. But it is important to note that no definition is possible unless the naming function is related to at least one other function which encodes some information the speaker assumes is already known to the hearer. Consequently, definition must be expressed at least at the level of a proposition.⁵ Because the naming function and the equivalence function must be present in order to make an (English) definition identifiable and effective as a definition, these two functions or constituents are said to be nuclear.

According to Trimble, a "semiformal" definition contains two of the constituents of a formal definition: the naming function and the contrastive function. For example, in

- we have the naming function: velarization; and the contrastive features: raising the back of the tongue--not lowering it; the back of the tongue--not the front; and so on. The constituent not found immediately in this definition is the classifying function: velarization is a type of (belongs to the set of) secondary articulations of vowel segments. This information is provided by the larger context in which the definition occurs--in this section of his book the author is talking about secondary articulations of vowel segments. It is not that information about the larger set is lacking--it is provided by the context.⁶ I would conclude, then, that the variation in immediate referential (and resulting grammatical) form between "formal" and "semi-formal" definitions is an etic one, and that emically the set information is there in the larger including context.

(8) formal definition = + naming + equivalent
(unknown) (known)

where equivalent = $\pm$ classifying/locating + contrasting.

3.2 INFORMAL DEFINITION

Trimble (1976:10) presents two types of informal definition: use of synonym and use of antonym. An informal definition by synonym is said to present a short word or phrase which may or may not be an approximate equivalent. Let us consider a situation where Jane sees vinculum in a list of words for a vocabulary test--the word does not appear in any context other than that list of words, which are otherwise unrelated (e.g. not from any common semantic field). Suppose Jane asks her older brother Bill what this particular word means and he replies, "vinculum means a connection." In this situation we do not know whether a vinculum is one particular type of connection (i.e. belongs to the taxonomic set of all connections), or alternatively, whether a connection is a type of vinculum.⁷ What Bill has done is to present "a connection" as an (approximate) equivalent for "vinculum". He has presented a commonly-known concept or identity for one which is unfamiliar to Jane. We can represent this as follows:

(9) informal definition =

Nuc	Identity _i	Nuc	Identity _j
+ Naming; unknown		+ Equivalent; known	

In this particular instance, Identity_i = vinculum; and Identity_j = a connection.

Informal definition by antonym is almost the same in terms of referential structure as definition by synonym. Suppose that another word in Jane's vocabulary list is obfuscation, and again she asks Bill what this one means. Let us suppose that last week's vocabulary list contained perspicuity and Bill capitalizes on this, replying, "The opposite of obfuscation is perspicuity." He is proceeding from the unknown, obfuscation, to the known, perspicuity. If we denote obfuscation as Identity_i, then perspicuity = Identity_j, where Identity_j = -(Identity_i). The rest of the representation remains identical. Consequently we may combine the two and have

(10) Informal definition =

Nuc	Identity _i	Nuc	Identity _j -(Identity _i)
+ Naming; unknown		+ Equivalent; known	

Comparing (8) and (10), we see that what both formal and informal varieties of English definition have in common are a naming function and an equivalence function, where the former is presumed to be unknown and the latter known. Thus, definition occurs when a speaker (or writer) creates a referential configuration in which these two role components are present. The difference between formal and informal subtypes of definition is determined by whether information about the larger including set and contrastive

components is given, or whether the unknown identity is just related to a second identity.

3.3 COHESION IN DEFINITION

Trimble (1976:10,11) presents what he terms "definition by stipulation," which amounts to limiting the context for which the given definition will hold. Suppose that Bill is a mathematician and when Jane asks him what vinculum means, he replies, "Well, in mathematics it means a bar that is drawn over some algebraic symbols to show that they're treated as a unit." But Bill's friend Sam, a medical student, is standing nearby and remonstrates, "But in anatomy, vinculum is a ligament that limits how much a body part can move." What both Bill and Sam have done is to provide referential cohesion features; that is, they have delimited two different universes of discourse or "worlds" for which a given definition of vinculum would hold true. These "worlds" are cohesion features in that they dictate what larger including set a vinculum may belong to, and what contrastive or classifying characteristics may be given. For mathematics, for example:

(11) Nuc	Identity (= vinculum)	(Margin)	Set (= symbol)
Naming	Mathematics	Classifying	gov. by U. of Math
Nuc	CF of Identity (= extends over algebraic symbols; indicates a single unit)		
Contrasting	gov. by U. of Math		

Since vinculum could still be paraphrased informally as "a connection" in either universe of discourse, it appears that stipulation of such a cohesion feature could be added to either the formal or informal variety of definition. I conclude that what Trimble terms "definition by stipulation" is not a different type of definition, but rather, a governing feature which may or may not be imposed to limit and govern other features of the definition.

4. GRAMMATICAL STRUCTURES USED TO ENCODE DEFINITION

So far we have looked at two types of English definition: formal and informal. These may be manifested in a variety of grammatical forms, showing that the referential structure of definition is to some degree independent of grammatical structure. In the following list of such grammatical structures, all are of clause level or above. As stated earlier, it is not possible to define something just by giving it a name; it must be related to some other entity. Such a relationship cannot be verbally expressed by anything lower than the level of an (implied or overt) proposition, which is the grammatical meaning for a clause or sentence (Pike and Pike 1977). Even in a dictionary definition where grammatical clauses may not be "complete" there is still a

proposition present: "X means/is Y".

Clause fragments (as found in a dictionary, for example):

make: to create; construct; form shape. To give a new form or use to.

Clause---simple: "Vinculum means a connection." "A triode is a vacuum tube."

Complex clause/sentence: "Malaria is an infectious disease characterized by cycles of chills, fever, and sweating, transmitted by the bite of the female anopheles mosquito."

Paragraph: A paragraph may encode what Trimble (1976:11,12) terms an "expanded definition." "Frequently expanded definition consists of a paragraph of information with the core generalization of the paragraph the initial definition (usually formal or semiformal)." Such a core generalization may be expanded in the paragraph by various rhetorical devices (e.g. explication, telling how something operates, classification, example, or visual illustration), which serve to give the hearer or reader further specifying information about the named entity, filling out his referential picture of it.

Paragraph complex/discourse: A grammatical chunk even larger than a paragraph could be employed to define an entity. Conceivably, the definition given in (1) could be the initial formal statement given in a chapter on triodes in an electronics text book. Subsequent paragraphs might further specify the features of a vacuum tube, a cathode, an anode, and a grid; with a final paragraph describing the operation of a triode in a larger system.

In conclusion, one language utterance may have one grammatical structure and another referential structure. Furthermore, that one referential structure may be expressed in a variety of grammatical forms. It follows that grammar and reference are to some degree independent of each other (and therefore, are not the same thing); but since it is also true that such a referential structure cannot be expressed with certain types of grammatical constructions, there is some degree of interrelationship between the two components.

Grammatical structure differs from referential structure in a variety of ways, two of which are (a) in the types of filler classes found: in grammar we have syntactic entities; in reference we have linguistic references to real-world (or hypothetical) identities. (b) The hierarchical relationships in grammar are strictly part-whole; in reference, they are both part-whole and taxonomic.

Referentially, English definition must have two constituents: a naming function and an equivalence function, where the former is assumed to be unknown to the hearer, and the latter known. Because these two functions must be related in order for a

definition to occur, definition must be expressed at least at the level of an (implied or overt) proposition.

NOTES

¹It is this sort of structure which Jones 1977 calls a "script".

²Items such as NP and Verb are further identifiable as syntactic entities by their occurrence in a linear sequential ordering among other such units in the speech output, as dictated by word order rules, for example. They may be further identified as syntactic by their occurrence in a system. For example, in (3) we have an equative verb, which occurs as part of a system of transitivity: transitive, intransitive, bi-intransitive, etc.

³There may be other, non-hierarchical, relationships of a logical nature between entities in referential organization. E.g. implies, presupposes, etc.

⁴Grammatically, it often proceeds from unknown to known--which is the opposite of most types of discourse patterns for "old" and "new" information.

⁵In Sec. 4 of this paper it will be seen that all the grammatical devices used to express definition are clause-level and above; and I assume here that the grammatical meaning of a clause is to encode a proposition (Pike and Pike 1977:Chapter 3).

⁶Trimble does state that the use of semi-formal versus formal definition depends on whether the information about the larger including set is either obvious, or unimportant to the subject (1976:9).

⁷One example of this sort used by Trimble is "An arachnid is a spider". Technically, the speaker is defining the set called "arachnid" by one of its members, "spider". However, this is not the reverse of the two initial constituents found in a formal definition because the speaker's purpose is not to locate or classify spider as a member of the set "arachnid". Although a sufficiently-knowledgable person would recognize that arachnids taxonomically include spiders, the speaker's purpose here is not to have his hearer think of it as an including set, but to think of it as an equivalent concept or entity. Thus, the correct formulation is as given in (10).

REFERENCES

- Abercrombie, David. 1967. Elements of General Phonetics. Chicago: Aldine Pub. Co.
- Jones, Linda K. Johnson. 1977. Theme in English Expository Discourse. (Edward Sapir Monograph Series in Language, Culture, and Cognition, supplement to Forum Linguisticum) In press.
- Pike, Kenneth L. and Evelyn G. Pike. 1977. Grammatical Analysis. (Summer Institute of Linguistics Publications in Linguistics and Related Fields). In Press.
- Trimble, Louis. 1966. The Rhetoric of Informational English. ms. Seattle: University of Washington.

QUELQUES ASPECTS D'UNE STYLISTIQUE COMPAREE DU
FRANCAIS ET DU HONGROIS A LA BASE DE LA LINGUISTIQUE
CONTRASTIVE

Jolande Kelemen

1. Depuis quelques années, on voit paraître en nombre croissant des ouvrages, des études et des articles consacrés à la stylistique comparée ou à la linguistique contrastive. L'intérêt suscité par ces nouveaux domaines de la linguistique---science à laquelle nous rattachons la stylistique---a des raisons diverses, en premier lieu les progrès de la typologie, mais aussi des raisons pédagogiques. Le descripteur d'une langue étrangère---de même que celui qui l'enseigne ou l'étudie---ne se contente plus d'examiner cette langue en elle-même, ou tout au plus en la comparant à d'autres appartenant à la même famille; il est curieux de connaître les équivalents des faits de langue étudiés dans sa langue maternelle, les divergences et les convergences des deux langues, afin de mieux comprendre le système et le mécanisme de la langue étrangère. Le linguiste ou l'enseignant bilingue ressent encore plus la nécessité de comparer les deux langues acquises généralement dès l'enfance, surtout s'il s'intéresse aux problèmes de traduction.

1.1. Qu'est-ce que la linguistique contrastive? Comment peut-elle se définir par rapport à la stylistique comparée? La linguistique ou plus exactement la description contrastive n'a pas encore défini ses méthodes. Evidemment, la pierre d'achoppement de cette branche de la linguistique appliquée est le corpus, car le seul moyen d'exclure la stylistique de ce domaine de recherche serait d'étudier un corpus représentant le degré zéro, non-marqué de la langue. Une grammaire traditionnelle dont les exemples sont en grande partie littéraires ne peut donc convenir à ces fins, alors que la grammaire générative, avec ses phrases simples, réduites à l'essentiel, paraît plus acceptable. Or, la grammaire générative elle-même n'a pas défini la norme, cette norme qui représenterait la langue neutre, exempte de tout fait de style. Il faut reconnaître toutefois qu'il semble presque impossible de définir une telle norme, puisque tout fait de langue résultant d'un choix représente inévitablement un niveau de langue et devient par suite fait de style.

Voici par exemple ce que disent à ce sujet les auteurs d'un remarquable article sur la norme, J. Authier et A. Meunier: "... On peut faire l'hypothèse qu'il existe une 'compétence moyenne', sous-tendant l'ensemble des énoncés admis par tous les locuteurs du français: les cas douteux seront alors, en principe, éclairés par les règles établies pour rendre compte des cas indiscutables..., un certain nombre de divergences irréductibles (variantes socio-culturelles, par exemple) étant renvoyées au domaine de la performance" (p. 52). Après avoir démontré, très

justement, que la plupart des grammaires génératives excluent les énoncés étrangers au parler du locuteur et les assimilent à des énoncés "qu'aucun locuteur ne peut reconnaître comme grammaticaux" (p. 60) les auteurs aboutissent à la conclusion suivante: "On ne peut signaler les lacunes concernant la reconnaissance des niveaux de langue, et les problèmes touchant à l'existence d'une norme implicite, dans les grammaires génératives actuelles du français, sans replacer ces faits dans le cadre général de la pauvreté des connaissances linguistiques actuelles en ces domaines. La nécessité d'un travail descriptif systématique dégageant, entre autres, le jeu des facteurs sociaux et des situations de communication, telle que la soulignent R. L. Wagner et B. Quemada apparaît comme toujours aussi pressante." (p. 61).

Il ressort de tout ceci que, si nous considérons la norme comme la "compétence moyenne", celle-ci grouperait les faits de langue; dès qu'interviendraient les facteurs sociaux et les situations de communication, on aurait des faits de style. Pour le moment, et en attendant la parution du travail descriptif systématique préconisé par les auteurs cités, une définition possible de la description contrastive semble être la suivante: Partir d'un problème de linguistique descriptive de la langue A, qui serait étudié selon une méthode proche de la grammaire générative, mais faisant abstraction de tout fait de style, puis confronter ce problème avec son équivalent dans la langue B, en acceptant l'éventualité d'une non-correspondance des niveaux grammaticaux (la structure qui relève par exemple de la pronominalisation dans A peut avoir pour équivalent, dans B, une structure relevant de la conjugaison).

Cependant, comme nous l'avons déjà dit, définir une norme en faisant abstraction de tout fait de style est chose difficile. Ce n'est donc pas par hasard que le travail descriptif systématique fixant la norme, les niveaux de langue et les situations de communication n'a pas encore été fait, et ce n'est pas par hasard non plus que les excellents pionniers de la description contrastive, L. Malblanc et Vinay-Darbelnet ont choisi le terme de stylistique comme titre de leurs ouvrages respectifs: "Stylistique comparée du français et de l'allemand" et "Stylistique comparée du français et de l'anglais". D'autre part, on sait que la stylistique, comme telle, est fortement contestée depuis déjà plusieurs années. Les linguistes répètent, depuis plus de dix ans, que la stylistique est morte, qu'elle n'a plus droit de cité. Or, parler de fait de langue en pensant à la "compétence moyenne" et de fait de style à propos de tout ce qui s'oppose à cette compétence parce qu'appartenant à un niveau de langue différent paraît acceptable, même si de la sorte le fait de style ne se limite pas uniquement à la langue littéraire, qui évidemment forme le corpus par excellence de l'analyse stylistique.

1.2 A titre d'exemple nous allons examiner un cas de pronominalisation par le pronom dit adverbial en, dont les équivalents contrastifs montreront la nécessité de distinguer le fait de langue du fait de style. La langue de départ est le français, la langue d'arrivée le hongrois. Étant donné le nombre limité de

ceux qui comprennent cette langue, les exemples hongrois seront entre parenthèses et leur omission n'altérera en rien l'intelligibilité du texte.

1.2.1. En, fait de langue. On sait que le pronom en pronominalise deux SN de fonctions différentes: SN en fonction d'objet direct et SN en fonction de complément circonstanciel.

SN complément d'objet pronominalisé par en est toujours indéfini: il est précédé d'un article indéfini ou partitif, ou encore d'un quantitatif (numéral, adjectif indéfini ou adverbe de quantité) qui se maintient dans la pronominalisation. Il exprime indifféremment un animé ou un inanimé:

Je cherche des livres, des hommes -- J'en cherche.
 Je cherche du vin -- J'en cherche.
 Je cherche beaucoup de livres -- J'en cherche beaucoup.
 Je cherche deux livres -- J'en cherche deux.

Si le syntagme à pronominaliser contient une épithète, celle-ci fait réapparaître dans la transformation l'article:

Je cherche un bon livre -- J'en cherche un bon.
 Je cherche du vin blanc -- J'en cherche du blanc.

En hongrois, l'équivalent de la phrase française à objet indéfini est donné par la conjugaison subjective, et la transformation pronominale se fait généralement avec effacement du pronom en structure de surface.

(Keresek könyvet, embeket -- Keresek.)

Le quantitatif se maintient en hongrois tout comme en français, de même que l'épithète, et ils sont tous deux marqués du -t objectif.

(Sok könyvet keresek -- Sokat keresek.)
 (Két könyvet keresek -- Kettőt keresek.)
 (Jó könyvet keresek -- Jót keresek.)
 (Féhér bort keresek -- Fehéret keresek.)

Notons, sans approfondir la question, que le pronom en peut se substituer à la séquence SN d'un verbe impersonnel ou de voici, voilà:

Il y a (voici) des enfants -- Il y en a, en voici.

Enfin, ajoutons encore que, dans les deux langues, il existe pour toutes ces transformations un assez grand nombre de restrictions; nous n'avons pas l'intention d'entrer ici dans ces détails (pour le français, cf. M. Gross, p. 29 *saa.*). Dans l'enseignement du français aux Hongrois, en complément d'objet est souvent traduit par un pronom emphatique (abból, belőle) impliqué par la forme subjective du verbe et généralement redondant.

Le syntagme de SN (ou éventuellement de SV) en fonction de complément circonstanciel a souvent un équivalent hongrois dans sa transformation pronominale:

Je viens de Budapest -- J'en viens. (Onnan jövök).
 Je suis désolé de vous quitter -- J'en suis désolé.
 (Bánkódom miatta).
 Il l'a recouvert d'un papier -- Il l'en a recouvert.
 (Betakarta vele).

Le groupe adverbial de SN peut avoir une valeur de "complément d'objet indirect" (correspondant à ce que le hongrois nomme

"állandó határozó"):

Il m'a parlé de son travail -- Il m'en a parlé.
(Beszélt róla).

Il se souvient de cette rencontre -- Il s'en souvient.
(Emlékszik rá).

1.2.2. En, fait de style. En complément d'object direct ne connaît pas de variantes stylistiques, puisque son emploi est obligatoire pour les animés comme pour les non-animés. Par contre, en adverbial présente des variantes qui reflètent l'usage individuel à l'intérieur d'un même niveau de langue. Il s'agit de l'opposition en/de lui de la langue littéraire, pour désigner des animés. Voici quelques exemples recueillis par J. Pinchon:

"La comtesse se réduisit bientôt à obtenir de lui que du moins il fît part de son projet à sa mère." (Stendhal, Chartr. 51).

"Théodore, la semaine suivante, en obtint des rendez-vous." (Flaubert, Trois contes, II, 593).

"... mais je ne parviens pas à faire de lui (Nerval) ce grand poète que nous représente Thierry Maulnier." (Gide, Journal, I, 25).

"Aimez monsieur de Rubempré, protégez-le, faites-en tout ce que vous voudrez, mais ne demeurez pas ensemble." (Balzac, Illusions, IV, 596).

Selon J. Pinchon (p. 86) ces emplois de en "pour les personnes montrent que l'on a tendance à préférer les régimes atones aux régimes toniques, les formes synthétiques aux formes analytiques", raisons auxquelles s'ajoute un facteur important: au XVII^e siècle encore, en désignait des personnes beaucoup plus souvent que de nos jours. Ainsi, ayant aujourd'hui une valeur d'archaïsmes, ces emplois appartiennent sans conteste à la langue littéraire, sans que toutefois l'opposition en/de lui corresponde pour autant à une opposition littéraire/non-littéraire, comme le prouvent les exemples cités.

Ce fait de style propre à l'usage individuel n'a aucun équivalent en hongrois: en stylistique comparée, il faudra partir de sa valeur d'archaïsme et chercher pour la langue d'arrivée un autre fait grammatical ayant également une valeur d'archaïsme.

D'ailleurs il existe en hongrois également des variantes stylistiques correspondant à en adverbial, mais d'une tout autre nature: il peut y avoir, dans l'usage individuel, effacement du pronom, car en hongrois la pronominalisation n'est pas toujours nécessaire; elle peut paraître redondante. Dans ce cas, l'usager de la langue choisit volontiers la solution plus économique, surtout dans la langue littéraire, moins soucieuse que l'usage moyen d'explicitement les faits de langue. Voici quelques exemples tirés du Passe-muraille de M. Aymé, avec entre parenthèses leur traduction hongroise, caractérisée par l'effacement du pronom hongrois correspondant à en:

"... Enfermé au triple verrou dans un cachot ombreux, Garou-Garou s'en échappa le soir même (14). (Bezárták háromszoros lakattal, a börtön egyik sötét zárkájába. De Mumus-Mumus még aznap este megszökött [22])."

"Malheureusement, la belle, Dutilleul en fut informé par Gen

Paul, était mariée à un homme brutal et jaloux (16). (Sajna, a szépség, amint Dutilleul Gen Paul-tól értesült, egy durva és féltékeny férfi felesége volt [24]).

"Ce fut l'après-midi de ce même jour qu'il devient amoureux ... Il en oublia aussitôt sa collection de timbres (16). (Ezen a délután történt, hogy beleszeretett egy szöke szépségbe ... Azon nyomban elfelejtette bélyeggyűjteményét [24]).

2.1. Les oppositions stylistiques telles que les précédentes, qui relèvent de l'usage individuel à l'intérieur d'un même niveau de langue, peuvent être considérées comme non-pertinentes du point de vue de la grammaire contrastive. Dans une deuxième catégorie, on trouvera les oppositions stylistiques qui représentent deux niveaux de langue différents à travers des faits grammaticaux plus ou moins identiques dans les deux langues. Ainsi, dans le roman contemporain, il existe une tendance générale à la concision, qui se réalise le plus souvent par un style nominal, c'est-à-dire l'emploi de phrases nominales brèves, souvent avec omission du déterminant. Ce style littéraire moderne (que l'on retrouve d'ailleurs aussi dans la prose romantique de V. Hugo), dont les faits grammaticaux sont donc la nominalisation et l'omission fréquente du déterminant, s'oppose à la langue parlée. On l'utilise aussi bien en hongrois qu'en français. Voici un exemple pris dans H. Bazin, La mort du petit cheval:

- "Avez-vous pensé que nous aurions des enfants?
- J'espère bien.

Demande inattendue. Réponse inattendue, instructive...

- Des enfants heureux, quelle revanche!" (p. 151)

Le présent historique figurant dans un corpus littéraire présenté au passé (simple) est également un fait de style commun aux deux langues. Il n'en reste pas moins qu'en syntaxe les faits de style de deux langues présentent rarement une équivalence grammaticale parfaite, surtout lorsqu'elles appartiennent à deux familles tout à fait différentes. En français par exemple, de tels faits de style, marquant une opposition littéraire/non-littéraire, sont le plus-que-parfait du subjonctif opposé au passé du conditionnel, ou l'inversion verbe-sujet opposée à la séquence progressive, ou encore la négation avec le simple ne opposée à la négation double. En hongrois, ces faits grammaticaux n'existant pas, il est évident que le niveau de langue à reproduire le sera par d'autres faits grammaticaux, de valeur semblable. Les faits de style plus ou moins équivalents sont donc à chercher en premier lieu dans le domaine de la lexicologie stylistique, des figures de mots et des figures de construction.

2.2. La troisième catégorie d'oppositions est incontestablement la plus intéressante: elle groupe les faits de style de valeur exceptionnelle, qui dominant tout le corpus et lui confèrent un caractère spécial. De telles oppositions stylistiques sont très rares, et elles se présentent souvent comme variantes à l'intérieur d'un même niveau de langue: la plus célèbre est le passé composé de L'Etranger opposé au passé simple habituel de la narration écrite. L'emploi du passé composé se réalise dans un corpus littéraire, mais qui est une reproduction de la langue

parlée, de sorte qu'il existe une certaine opposition de niveaux de langue. Du point de vue de la stylistique comparée, un tel fait de style est extrêmement intéressant, parce qu'inexistant dans la langue d'arrivée. Le hongrois ne connaît qu'un seul passé: cela revient-il à dire que notre corpus est intraduisible ou que la traduction faussera la pensée de l'auteur?

Comme tout fait de style de ce type, le passé composé, temps principal de la narration dans *L'Etranger*, ne peut figurer que dans un contexte qui lui est propre, porteur du même message que lui. Ce passé composé est soumis aux restrictions suivantes: il ne s'emploie que dans un texte à la 1^e personne du singulier, proche du journal intime; c'est le récit d'un personnage à culture superficielle, pauvre en pensées et en sentiments; le récit a une valeur de témoignage et d'authenticité, justement parce qu'il reproduit la langue parlée.

Les autres moyens stylistiques employés par l'auteur dans le même but que le passé composé sont les suivants: pauvreté du lexique, absence d'adjectifs; verbes à indice de fréquence très élevée, comme être, avoir, rester, laisser, passer, attendre, regarder, penser; phrases courtes; style indirect fréquent reproduisant des conversations banales pour les rendre encore plus effacées; abondance des lieux communs et des phrases sentencieuses. A propos du style indirect, notons que les verbes déclaratifs qui l'introduisent sont également à fréquence élevée: dire (nettement le plus fréquent), demandeur, répondre, expliquer, observer, murmurer, etc. Les phrases au style indirect sont généralement courtes, réduites aux deux termes essentiels. En dehors du passé composé, le temps prédominant est l'imparfait: à eux deux, ils représentent les 75 à 80% du nombre des verbes conjugués, le reste étant réparti entre les autres modes et les autres temps, avec très peu de variété.

Si en hongrois l'opposition passé simple/passé composé n'existe pas, les faits linguistiques et stylistiques énumérés existent et permettent une traduction sinon parfaite, mais somme toute assez fidèle du texte français. Ceci est donc en contradiction avec l'affirmation de Robbe-Grillet, selon laquelle il suffirait de remplacer la 1^e personne du passé composé de ce livre par la 3^e personne du passé simple "pour que l'univers de Camus disparaisse aussitôt, et tout l'intérêt de son livre" (p. 50). Il est très simple de prouver l'impossibilité de changer uniquement la personne et le temps du récit, comme le suggère Robbe-Grillet. Rien que la première phrase: "Aujourd'hui maman est morte" résiste à ce genre de transformation. On ne pourrait écrire: "Aujourd'hui, sa mère mourut", le passé simple et l'adverbe de temps aujourd'hui étant incompatibles. Pourrait-on écrire alors: "Ce jour-là, sa mère mourut"? Le passé simple, dans la première phrase d'un récit, n'est pas d'un effet très heureux, il semble préférable de se servir dans ce cas d'un plus-que-parfait d'introduction. On aurait alors une solution acceptable: "Ce jour-là, sa mère était morte". Comme on le voit, il est impossible et insuffisant de ne changer que le temps et la personne d'un ouvrage en prose donné. Si c'était suffisant, le

roman de Camus tel qu'il est serait intraduisible. Or, comme toute opposition stylistique n'est valable que dans le contexte qui lui est propre, ce passé composé est en rapport étroit avec tous les facteurs linguistiques et métalinguistiques indiqués.

En conclusion, l'opposition stylistique utilisée dans la langue de départ n'a pas été traduite, parce qu'elle était en elle-même intraduisible, mais les facteurs propres à l'environnement et en rapport étroit avec cette opposition ont pu être traduits.

Ceci prouve que les faits de style importants du corpus de la langue de départ peuvent et doivent être représentés dans la langue d'arrivée, même s'ils semblent être en eux-mêmes intraduisibles. Cela signifie également qu'un fait de style ne peut jamais être isolé, à moins qu'il n'indique une rupture dans le texte. Peu importe donc que la totalité des moyens stylistiques d'un corpus constituant un certain style et représentant un certain niveau de langue soit traduite intégralement dans la langue d'arrivée: tel fait de style de cet ensemble peut s'effacer en surface (ainsi le passé composé de *L'Etranger*), le message exprimé par les autres faits de style reproduits en langue d'arrivée n'en demeure pas moins fidèle au corpus. Dans ce sens donc, il n'existe pas de faits de style intraduisibles.

3. Les problèmes soulevés par l'analyse des oppositions stylistiques montrent la nécessité de recenser les critères grammaticaux qui caractérisent la langue littéraire, et de définir de cette manière le niveau de la langue littéraire, assez négligé par la linguistique moderne. Cependant, la réalité linguistique nous oblige à tenir compte de plus en plus de l'importance croissante de l'oral par rapport à l'écrit, par suite d'un certain mélange des niveaux de langue, facilité d'ailleurs par des facteurs sociologiques. C'est ainsi qu'en français, tout comme en hongrois, les compromis sont de plus en plus nombreux en linguistique, et l'ancienne norme stylistique, fondée sur une littérature écrite et éloignée de la langue vivante, cède le pas à une norme plus fonctionnelle, plus proche de la réalité.

De nos jours, on voit se multiplier les romans ou récits dans lesquels se côtoient constamment deux niveaux de langue, l'un dans la narration, l'autre dans les dialogues, et ceci vaut aussi bien pour le hongrois par exemple que pour le français. Dans de tels cas, la stylistique comparée a pour tâche d'étudier les moyens stylistiques permettant de caractériser et d'opposer les deux niveaux de langue en français et en hongrois, puis de les comparer entre eux. A titre d'exemple, nous présentons quelques phrases tirées de la nouvelle "Les bottes de sept lieues" de M. Aymé (pp. 172-173):

"La nuit était presque tombée. Peu rassuré, le petit Baranquin partit en courant. En l'attendant, le chef sortit un papier de sa poche et l'examina en fronçant le sourcil.

- Fermez ça, bon Dieu, dit-il à Huchemin et à Rogier qui parlaient haut. Vous voyez pas que je médite, non?

Bientôt, on entendit claquer les galoches de Baranquin qui rejoignait au pas gymnastique. Au cours de sa reconnaissance, il

n'avait rien vu de suspect et le déclara tout innocemment ... Frioulat prit ses compagnons à témoin.

- J'ai pourtant l'habitude de commander, dit-il, mais des cons comme celui-là, j'en ai encore jamais vu.

Les compagnons comprenaient parfaitement le reproche et le trouvaient justifié ...

Après un silence, Antoine fit observer:

- Du moment qu'il n'a rien vu, il le dit. Je vois pas pour-quoi on lui en voudrait.

Huchemin, Rogier et Naudin approuvèrent à haute voix et le chef en fut un peu troublé.

- Alors, quoi, si on s'occupe de ce qui est vrai, y a plus moyen de rien faire, dit-il.

Antoine ... voulut faire amende honorable, mais aux premiers mots qu'il dit, Frioulat le prit à partie.

- Ta gueule, toi. Au lieu de venir flanquer l'indiscipline dans la bande, t'aurais mieux fait de rentrer chez ta mère."

Dans le récit du narrateur, la langue littéraire est explicitée moins par le lexique que par des faits syntaxiques propres à ce niveau de langue, avant tout l'emploi du passé simple comme temps principal de la narration, puis par un adjectif détaché en tête de phrase: "peu rassuré", par la non-répétition du pronom personnel sujet en coordonnée: "il n'avait rien vu de suspect et le déclara tout innocemment; - les compagnons comprenaient ... le reproche et le trouvaient justifié." Ces faits de style sont loin d'être les plus typiques de la langue littéraire, puisque nous n'avons dans ce bref corpus ni imparfait du subjonctif, ni inversion VS, ni négation par simple ne, etc. Mais le style des dialogues est d'autant plus caractéristique de la langue familière, voire populaire: d'abord, le lexique, avec des termes d'argot grossiers, tels que: "con, gueule", ou des expressions analogues comme "fermez ça", ou encore des locutions relevant de la langue parlée, comme "du moment que; alors, quoi". Les faits syntaxiques reflètent tout autant ce niveau de langue: élision de certaines voyelles, comme u dans tu: "t'aurais mieux fait", ellipse du pronom il de il y a: "y a plus moyen", effacement du ne de négation: "Vous voyez pas, - j'en ai encore jamais vu, - je vois pas pourquoi", enfin l'emploi général de on pour nous.

En examinant la traduction hongroise de ce texte (qui ne sera pas donnée ici), on constate que la différence des deux niveaux de langue est beaucoup moins nette qu'en français, et se réduit uniquement au lexique. En effet, le fait de style le plus typique de la langue littéraire, le passé simple, n'a pas d'équivalent en hongrois, et tous les faits syntaxiques employés dans la langue familière ou populaire font défaut dans le texte hongrois; tout au plus remarque-t-on l'élision de la voyelle i de miért 'pourquoi'. Par contre les moyens lexicaux utilisés sont riches et variés (fogjátok be a szátok, - az istenfáját, - ilyen marhát, - pofa be).

Tout ceci ne signifie certes pas que les moyens syntaxiques propres à la langue littéraire, et même---dans une mesure moindre

--- à la langue familière, n'existent pas en hongrois. Mais ils sont certainement moins nombreux qu'en français. Donc, en définissant les moyens syntaxiques pouvant être considérés comme faits de style, en leur apposant des étiquettes telles que: archaïsmes, style précieux ou autres, on pourra certainement démontrer les équivalences stylistiques entre les deux langues. D'autre part, une analyse de ce genre prouve aussi que la norme linguistique contemporaine se situe entre les deux niveaux de ce corpus, mais beaucoup plus près de la langue littéraire que de cette variété de la langue familière.

Les quelques réflexions qui ont été faites ici sur la stylistique comparée nous suggèrent la conclusion suivante: les recherches devraient se poursuivre à deux niveaux. D'une part dans une description contrastive des deux langues qui, après avoir défini la norme grammaticale, écarterait d'emblée les faits de style et permettrait leur regroupement systématique; d'autre part dans la définition et l'étude de la langue littéraire, et ceci aussi bien en français qu'en hongrois. Un tel travail permettrait permettre l'élaboration d'une stylistique comparée du français et du hongrois plus approfondie et plus moderne que celles qui ont paru jusqu'à ce jour, et dont la place entre la grammaire et le stylistique contrastives est assez difficile à définir, malgré leur utilité incontestable. En même temps, il constituerait un apport de valeur aux recherches de littérature, de linguistique et de stylistique des spécialistes des deux pays.

BIBLIOGRAPHIE

- Arrivé, M. "Postulats pour la description linguistique des textes littéraires". Langue française, La stylistique, 3 sept. 1969, pp. 3-13.
- Authier, J. et A. Meunier. "Norme, grammaticalité et niveaux de langue". Langue française, 16 déc. 1972, pp. 49-62.
- Aymé, M. Le passe-muraille (éd. Poche). Paris, 1943. Traduit en hongrois par B. Lázár Endre: A faljáró, Budapest 1970.
- Barrier, M. G. L'art du récit dans L'Etranger d'Albert Camus. Paris, 1962.
- Bazin, H. La mort du petit cheval (éd. Poche). Paris, 1950.
- Camus, A. L'Etranger (éd. Poche). Paris, 1957. Traduit en hongrois par A. Gyergyai: A közöny, Budapest, 1947.
- Gáldi, L. Précis de stylistique française (Cours polycopié). Budapest, 1967.
- Gross, M. Grammaire transformationnelle du français. Paris, 1968.
- Kelemen, J. "La pronominalisation par en et ses équivalents hongrois". Études finno-ougriennes XI, pp. 73-81.
- Malblanc, L. Stylistique comparée du français et de l'allemand. Paris, 1961.
- Pinchon, J. "Histoire d'une norme, emploi des pronoms 'lui, eux, elle(s), en, y'". Langue française, 16 déc. 1972, pp. 74-87.
- Renaud, A. "Quelques remarques sur le style de L'Etranger". French Review 30, 1957, pp. 290-296.

Robbe-Grillet, A. Pour un nouveau roman. Paris, 1963.

Vinay, J. P., et J. Darbelnet. Stylistique comparée du français et de l'anglais. Paris, 1958.

THE MEANING OF NOEL COWARD'S *VENICE*

Adam Makkai

University of Illinois at Chicago Circle

1. The text:

Last Wednesday on the *Piazza*
Near San Marco's rococo *duomo*,
I observed *una grassa ragazza*
With a thin, Middle Western *uomo*.

He was swatting a *piccola mosca*
She was eating a chocolate *gelato*,
While the orchestra played (from *La Tosca*)
A flat violin *obbligato*.

The stared at a dusty *piccione* -
They spoke not a single *parola*;
She ordered some *tè con limone*,
He ordered an iced *Coca-Cola*.

And while the *tramonto del sole*
Set fire to the *Grande Canale*,
She scribbled haphazard *parole*
On a glazed *cartolina postale*.

2. The Problem:

- a) What linguistic explanation are we able to provide about a writer's ability to generate essentially English sentences, capped with Italian rhymes?
- b) Are there two 'deep structures' involved? If the answer to (b) is 'yes', where and how does the proposed DS signal to the surface structure to 'interpret' the line-final word(s) not according to English, but according to Italian phonology?
- c) Is there only one deep structure involved? If the answer is 'yes' to (c), where does the single underlying deep structure provide the signal to the surface structure and the phonology to switch the code?
- d) Is the 'English' part of the poem the author's 'competence' and the Italian rhymes his 'performance', or the other way round?
- e) Is the poem in toto a matter of unusual performance, cleverly superimposed on an essentially identical deep structure?
- f) Could it be that the deep structure-surface structure

dichotomy is simply irrelevant to this poem, and so we must seek an alternate explanation?

g) Could the stratificational model account for this poem by suggesting that the poem is a code-switched realization of the same set of sememic reticula? Is such an explanation not basically similar to (c) outlined above, with only a minor difference in terminology?

g) Even if one should choose to take the stratificational explanation over the transformational one, JUST WHERE EXACTLY DOES CODE-SWITCHING OCCUR and HOW IS IT CARRIED OUT?

2.1

There is no easy answer to these seven questions which, if gone into in detail, could amount to a vast study on code-switching from a variety of points of view, including neurology, and sociopsycholinguistics.

Whether the poem is BASICALLY in English or in Italian can, I believe, be relatively easily decided by rewriting it once in English, and once in Italian. The rhymes - i.e. the fact that this text is a poem - could only be kept if the English material before the rhyming words would allow itself to be put in Italian which won't work because possessives, such as San Marco's and Adj+N combinations such as rococo duomo would have to be turned around to yield il duomo rococo di San Marco thereby changing the final word and thus upsetting the rhyming scheme. Let us, then, suppose that the poem is basically in English with the rhymes merely added in Italian for some sort of special effect. If this hypothesis is correct, we should have no difficulty rewriting the poem all in English:

Last Wednesday on the market place
Near San Marco's rococo house (church),
I observed a fat girl
With a thin Middle Western Man.

He was swatting a small fly,
She was eating a chocolate ice cream,
While the orchestra played (from La Tosca)
A flat violin counterpart. (Or 'obbligato').

They stared at a dusty pigeon
They spoke not a single word -
She ordered some tea with lemon
He ordered an iced Coca-Cola.

And while the setting of the sun
Set fire to the Grand Canal,
She scribbled haphazard words
On a glazed post card.

The rhymes are, of course, lost, and with them the author's

purpose of writing the poem bilingually in the first place. The poem COULD, of course, be rewritten in Italian as well; minor syntactic adjustments would have to be made to accommodate the word order conventions of Italian.

2.2

Let us approach now the poem from the 'literary' point of view, exploring, as it were, the 'stylistics' and the 'message' of it to see if these might shed some light on the universe of discourse this piece belongs to allowing us to penetrate the grammar of the poem from 'above', as it were.

I think we can easily agree that we are not dealing with a profound or a great poem in any sense of the word, but rather with a spoof, the kind of fun-poking at human foibles that Coward, both as a movie director and as a writer, has been well known for. The poem gives us certain clues as to the author's point of view and his intentions: the man is not a Japanese or a German tourist, nor is he British. He is a Middle Western uomo, that is, an American on a Venetian holiday with his fat - i.e., not irresistibly attractive - girl friend. Coward, the British humorist, observes the couple and comments on their comically common-place and rather dull behavior in the overpublicized and overromanticized setting of the famous square in front of Saint Marc's, a scene frequently seen on travel agents' posters. One wonders actually why Coward didn't add a couple of additional stanzas drawing a caricature of the famous Venetian gondola and its ruseful skipper, the tip-hungry gondolier: he could have got the couple wet falling into the canal as the thin Middle Westerner ushers his obese bride out of the gondola, etc. Apparently four stanzas were all he wanted to make his sketch, and perhaps adding other stanzas would have been too much. It is fairly clear, however, that he is MAKING FUN OF THE AMERICANS while at the same time giving the connoisseur's cunning smirk about Venice itself; here the sophisticated European is WARNING THE AMERICANS that the place is over-rated and not really so glamorous, after all. His attitude is not one of criticism or of reproach, he is more the passive observer (stanza 1, line 3); and by the time the dramatic personae are introduced (Stanza 1, line 3 and 4) he, the observer has already put HIMSELF into the same ridiculous situation as those he observes: he was on the piazza, not the market place, near San Marco's rococo duomo, not his church, or house. The observer is an integral part of the act of observation, overtly and admittedly, and hence the mild ridicule and the warning to the Americans is, at the same time, self-irony and self-warning as well. ('What on Earth am I doing here if this is the kind of dull scene I am going to be witnessing...'). He, the Englishman, is just as 'estranged' from the overpublicized romantic scenery as are the the American tourists, who are the 'jokers' in this little scenario primarily because they don't seem to realize how utterly out-of-place they are in Venice with Venice, of course, being the 'culprit' for offering such dull amusement as a dusty pigeon, iced tea, the American invention, Coke, which has become so universal, that it can be pronounced in flawless Italian as *una Coca-Cola*, rhyming perfectly with *parola*. (The

underlined words in the original text, San Marco's, rococo, and La Tosca are Italian words that the English and the Americans have adopted and which can be pronounced both according to English or Italian phonology; Coca-Cola is an English-based internationalism receiving as many foreign phonological realizations as it has commercial adoptions the world over, with such internationalisms usually being 'bent' towards whatever the host-speakers interpret as 'cool', i.e., Latinoid.)

The sequence of 'events' shows the thin Middle Western Man and the fat girl as a couple who are either bored with one another, or who do not really belong to each other, having perhaps only met recently with not much chance of a great romantic love. The description moves from 'he' to 'she' (stanza 2); then twice 'they' (stanza 3); then the order is reversed: 'she' and 'he'. An act by 'she' closes the poem. Whether the 'camera' focuses on him or her, it also gives us a 'cut' to the side: first the orchestra, then the dusty pigeon, and finally the setting of the sun over the Grand Canal. The last stanza is a coda in a sense: the setting sun, while suggestive of the end of the day, also suggests the falling of the curtain on the little scenario, and the post card she is scribbling haphazard words on, tie her (perhaps also him) in with whoever was left behind, probably in the Midwest of the U.S.A.

We learn in stanza 2 that he is active (he swatted a tiny fly) while she eats - this duality of behavior is underscored by the 'flat violin obbligato' from one of the world more sentimental (and somewhat overrated) operas, La Tosca. When they are together (stanza 3) the COUPLE DOES EXACTLY NOTHING, i.e., they are actively engaged in doing nothing ('they stared at a dusty pigeon, they spoke not a single word'). Then they both fill their stomachs again; she, with more refinement, orders tea with lemon, he orders an iced coke. The 4th stanza, as mentioned above, lets the curtain fall with the setting of the sun and the post card acts as the *envoi* calling back home, and therefore to us, the readers, the end of the scenario. (The poem was originally published in 1969 in the New Yorker.)

2.2.1

The Italian rhymes (replete with La Tosca and Coca-Cola, the first already international and the second an English-based internationalism) can now be seen as fulfilling a BASIC FUNCTION in the entire poem: without them we would not get the sensation of being POTENTIAL SUBJECTS TO RIDICULE when we are out of our own NATURAL LINGUISTIC MILIEU. These people are not at home on Saint Marc's Square in Venice, and the local culture ('flat violin obbligato') does not embrace them with the best it can offer. 'They spoke not a single parola' either means that they do not speak Italian, or that the trip has got them sufficiently down so that they no longer care to converse even in English with each other. What they do instead is rather atypical of the ideal Venice one thinks of: staring at a dusty pigeon and swatting a fly, eating ice-cream and drinking a coke can be done anywhere in the world including any MacDonald's stand anywhere in the U.S.A. Whatever 'haphazard parole' she may be writing on her local post cards probably exaggerate the trip: they are 'glazed', a word that alliterates and readily associates with

glitter and, as we know, not all that glitters is gold. (Read: not every trip to Europe is as glamorous as the travel agent's ad might have you believe.)

The role of the rhymes is, then, strategic-functional: they are the constant reminders at the end of each line of the CULTURAL DISPLACEMENT of the participants in this four-act mini-drama.

2.2.1.1

The author could have written a brief prose paragraph about 'cultural displacement' of Midwestern Americans in Europe, and the 'intellectual message' of such a paragraph would be easily understood by any reader. The present poem, however, while informing us implicitly about 'cultural displacement' explicitly PERFORMS for the reader the very fact of cultural displacement: as each sentence and each line, begun in English, carries us toward an expected English noun, NP, or N compliment (out of the 16 Italian rhymes 6 are complex NPs, *una grassa ragazza*, *piccola mosca*, *La Tosca*, *tè con limone*, *Grande Canale*, and *cartolina postale*, one is an adjective, *gelato*, and counting English-based Coca-Cola a simple N, the rest are simple nouns) we are sixteen times subjected to the 'culture shock' the fictionalized foreign traveler feels. This observation is, of course, a direct consequence of the very nature of poetry, whether of an elevated, serious nature, or of the light *vers de société* type, to which *Venice* undoubtedly belongs, since a poem, from Greek ποίησις 'do', 'make', is always more than its meaning given in prose paraphrase: it is as much of a HAPPENING for the reader who lives through it while reading it as it was for the poet who wrote it. In poetry, typically, the language user reverts to that archaic state of language use which Fónagy 1974 calls the primitive arche-language, a cognitive state in mankind's intellectual development in which language was much less abstract than today and much more of an iconic recapitulation of events in the real world. (The reader is invited to compare this observation with Chapter 10, 'The Origin of Language' of Bolinger 1975 and Chapter 2 of Halliday 1975.)

From a literary point of view, a significant part of the meaning of this poem is what it DOES to the reader; and what it does to the reader is EXPOSE HIM SIXTEEN TIMES to the actual crossing of a cultural and linguistic barrier moving us back and forth, as it were, between the English speaking world and Italy. Coward may have realized that one could easily tire of this which may partly explain why he did not enhance the ridicule with a possible gondola scene, or something similar.

2.3

But from a linguistic point of view, if an integral part of the meaning of the poem is what it DOES to the reader, - i.e., move him back and forth between English and Italian - than a major part of the poem's meaning must be its SPECIAL PERFORMANCE QUA POEM. Yet this special performance qua poem co-exists simultaneously with the recoverability of the basic syntax and semantics of the poem which can be expressed either in homogeneous English (as given above) or in Italian. In a strange sense, then, this poem is actually impossible and shouldn't exist at all. On the one hand, its 'meaning' is derivable

from its syntax and the dictionary definition of each lexeme in the text, whether English or Italian; on the other hand this legal 'meaning' misses the point of the poem entirely which is, as we have seen, that the reader must be shifted back and forth between *Italica* and *Anglica* and be made fun of (while making fun of) the protagonists in the tiny play. We are additionally puzzled by the fact that here apparently ONE SINGLE UNDERLYING SYNTAX is capable of carrying English material on the one hand, and Italian on the other. Strictly speaking the two languages are not entirely equivalent here. The English, which determines the shape of the sentences, and of which there is quite simply more per stanza, definitely dominates the text; secondly, the rhymed words, by virtue of being in the sensitive position of being the rhymes, all seem to fall into the same syntactic category of noun or nominal group. A noun, or a nominal group is always more reified, more 'tangible', than a verb or a verbal group, it is more 'concrete'. It can be compared to a point of arrival at the end of a journey: the English speaking travellers arrive in Venice, they are in Italy, where the objects they touch, consume, or are surrounded by, either visually or aurally, are (in) Italian. In other words the TRIP is in English, but the TRAIN STATIONS are in Italian sixteen times throughout the four stanzas. Could it be that the syntax itself, which prepares these Italian endpoints of arrival, was so conceived by Coward in its very Englishness, as to reach culturally and semantically pregnant nouns and nominal groups which can only deliver their intended effect if they are code-switched? Quite possibly. But if this is the case, we must be ready to face the consequence that some of the English syntax of the poem was occasioned by the anticipated Italian rhyme. It is well known poetic practice on both sides of the Atlantic to be 'pregnant with a promising rhyme' and then to construct a poem around that rhyme (Cf. Makkai 1973).

2.3.1

In order for a transformationalist deep structure derivation to be able to account for just monolingual poetry alone, where by poetry I mean rhyming and meter in dominant position over the ordinary sentence syntax, the deep structure would have to be so adjusted and re-explained theoretically, as to be able to take directions from the phonological component of the grammar. It is, strictly speaking, not a responsible statement to make that this is intrinsically impossible. The way deep structure has been conceived of up til recently, however, it is difficult to see how such a partial subordination of the DS to the phonology would be accomplished. Part of the difficulty is the unidirectional nature of DS derivations which map onto surface structures from left to right which, then, are interpreted phonologically, again from left to right. What would be needed would be a set of RETRO-REWRITE RULES, or some such device, that would allow the phonological component to map backwards onto the surface structure which, in turn, would map backwards onto the deep structure. I can think of no inherent logical reason why such a model would be impossible though I am not aware of such rules ever having been proposed in writing. (Barbara Hall Partee ((personal communication)) apparently gave an oral paper on 'reverse rewrite

rules' but has not published the paper.) Additional problems would arise since the TG theorist would have to account for the author's having chosen a rhyme before having chosen a sentence structure that would accommodate that rhyme in whatever syntactic position was appropriate for it. It seems that whatever solution one might come up with would be highly suspect of being arbitrary and ad hoc in the extreme.

Given the fact that Italian and English are both Indo-European languages with considerable genetic and typological affinity, the same-deep-structure-hypothesis could perhaps be maintained and justified up to a certain point. Given the appropriate *IE reconstructions, the poem could actually be rewritten in Proto-Indo-European and both the English and the Italian version could be 'derived' from this (historically) underlying *PIE poem by a long and complicated set of 'derivational rules', in the spirit of Lightner 1975. (For a reply to Lightner's suggestions, see Lockwood 1976). Only an extremely erudite Indo-Europeanist could equate the notion of 'same underlying deep structure' with a reconstructed *PIE version of the text, and Noel Coward is not a historical linguist.

2.3.2

Useful models of the stratificationalist account of poetry and bilingualism are available in Makkai 1973 and Paradis 1976. The bilingual individual has in his memory parallel sets of phonological, morphological, lexical, and sememic material, which he can switch and correlate by virtue of certain extralinguistic considerations housed in that part of his cognitive system which has been called 'gnostemics', 'hypersememics', or the 'pragmo-sememic interface'. The stratificational model would allow one to speculate about how *VENICE* was conceived in some such manner as follows:

(1) Coward knew the basic plot of his poem before he began writing it. The idea was not yet entirely clear in his mind, and the sentences as they now actually appear in print weren't ready yet as he began writing. He probably decided, however, that the main sentence structure would be English and that the rhymes would be in Italian. This must have been in direct response to the CONTEXT OF SITUATION which occasioned the poem.

(2) Being probably stronger in English than in Italian, he picked nouns and nominal groups to be the Italian rhymes.

(3) Being nevertheless knowledgeable in Italian, he knew that the words *piazza-ragazza*, *duomo-uomo*, *mosca-Tosca*, *gelato-obbligato*, *piccione-limone*, *parola-Coca-Cola*, *sole-parole*, and *canale-postale* were good rhymes. It is not possible at this stage to reconstruct just in what order these occurred to him, but by rereading the English with the Italian rhymes left off we get: Last Wednesday on the _____ near San Marcos rococo _____ I observed _____ with a thin Middle Western _____. He was swatting a _____, she was eating a chocolate _____, while the orchestra played (from La Tosca) a flat violin _____. They stared at a dusty _____, they spoke not a single _____; she ordered some _____, he ordered an iced Coca-Cola. And while the _____ set fire to the _____, she scribbled haphazard _____ on a glazed _____.

(4) What we now have are two matrices, the English sentences with the gaps to be filled in, and the Italian rhymes asking for

sentences to be carried by, to be housed in.

(5) As a 'bilingual' in this situation, Coward knew, of course, the 'meanings' of the Italian words as well as the meanings of the English words which, as we saw above, are easily translatable into one another. The translation of the words of L₁ into the words of L₂ seems to occur by taking the corresponding sememe of L₁ and comparing it to the sememe of L₂ to see if they fit. In the case of *tè con limone* = 'tea with lemon' and *Grande Canale* = 'Grand Canal' the translation process is facilitated by the fact that the words involved are close cognates, but no great difficulty is encountered by such non-cognate material as *una grassa ragazza* = 'a fat girl' and *una piccola mosca* = 'a small fly' either, as the lexemes (and the corresponding sememes) involved are common in both cultures and exist essentially within the same scope of boundary. Serious problems of translation always occur when the item to be translated is unique to one or the other of the cultures, i.e., imagine Coward's having had to deal with *hominy grits*, *bubble and squeak*, *pecan pie*, and/or *prosciutto*, *frutti di mare* or some such quasi-proverbial, institutionalized Italian attitude as *dolce far niente*. We can hypothesize, then, that Coward's task was a relatively easy one, insofar as he dealt with CULTURALLY RELATED SEMEMES whose respective Italian and English lexemic realization could be accomplished rather rapidly, by using the sememe more or less as an unconscious token merely rather than a laborious technical definition. Common lexemes such as *sun*, *moon*, *mother*, and *father* are immediately and unconsciously translatable into whatever languages one is dealing with, because the sememes of which they are the lexemic realizations came into our consciousness not by technical definitions but by primary bio-physical contact during early childhood. The reader should try to translate the English terms *filibuster*, *gerrymander*, *lame duck President*, and *wild cat strike* into the foreign language of his choice, right after translating *sun*, *moon*, *mother* and *father* to appreciate the difference between what I propose to call a BIO-PSYCHO-SEMEME and a SOCIO-TECHNO-SEMEME. The only socio-techno-sememe in *VENICE* is the word *obbligato*, a technical term pertaining to counterpoint in music.

(6) Coward, then, wrote the poem in two languages at once, displaying double competence and double performance, localizing the point of the code-switch always in rhymed, line-final position, with these positions always coinciding with a noun or a nominal group.

(7) Coward must have been counting on a bilingual audience when he wrote the poem. (Bilingual in the sense that the reader, basically an Anglo-American, would be able to understand the Italian words in the rhymed positions.) The poem falls rather flat for a reader who has to look up every word in an Italian-English dictionary and who doesn't know how to pronounce them. This is essentially an upper-middle class elitist attitude; this is not working class humor, but bourgeois humor.

3.

The meaning of this poem can only be approached if we take all of these considerations into account simultaneously. As it is by now plainly evident, the author's cognitive system, his 'gnos-temics' or his 'hypersemology' is the crucial area where everything

else must be co-ordinated. These elements, roughly, are:

- (a) The plot,
- (b) The syntax,
- (c) The knowledge of the setting,
- (d) Familiarity with the sociology of the participants,
- (e) Knowledge of the syllabic pattern of English,
- (f) Knowledge of the Italian lexemes, their meanings, and the fact that they are eligible rhymes,
- (g) Anticipation of the derived effect on Anglo-American readers.

There could be many more; these few points are the inevitable necessities. The dynamics of shifting the reader between English and Italian has been discussed before and falls into the areas of a-b-c above.

3.1

Creative language use is extremely complex and does not readily yield itself to routine grammatical analysis of no matter what vintage and no matter what theoretical bent. Linguists have been reluctant to look at creative language use precisely because it poses so many problems and difficulties. Far from being fanciful and idle however, the study of poetic language and imaginative language use is perhaps the most important aspect of linguistic research, because people do indulge in creative language use all the time, whether wittingly or unwittingly. It is also to be expected that once linguistics faces up to the task of dealing with the unusual, it will have a much better chance of dealing with the ordinary, than the other way around.

REFERENCES

- Bolinger, Dwight
1975 *Aspects of Language* (revised edition), New York, Harcourt Brace Jovanovich.
- Fónagy, Iván
1975 *Füst Milán: ÖREGSÉG. (A költő hangja)* (Analysis of the poem 'Old Age' by M.F.) Budapest, Akadémiai Kiadó. (English translation by Adam Makkai in press.)
- Halliday, M.A.K.
1975 *Learning How to Mean*. London, Edward Arnold.
- Makkai, Adam
1973 'The Transformation of the Turkish Pasha Into a Big, Fat Dummy' in *Readings in Stratificational Linguistics*, A. Makkai and D.G. Lockwood (Eds.), University, University of Alabama Press.

Paradis, Michel
1976

'The Stratification of Bilingualism' in
The THIRD Lacus Forum, R.J. Di Pietro and
E.L. Blansitt (Eds) Columbia, Hornbeam Press,
pp. 237-247.



CONGRESS SERIES

With its Congress Series, Hornbeam Press, Incorporated will publish papers given at meetings, symposia, congresses, and the like.

Already published in this series

The First LACUS Forum

The Second LACUS Forum

The Third LACUS Forum

The Fourth LACUS Forum

Language and Linguistic Problems in Africa

Other volumes are planned for the near future